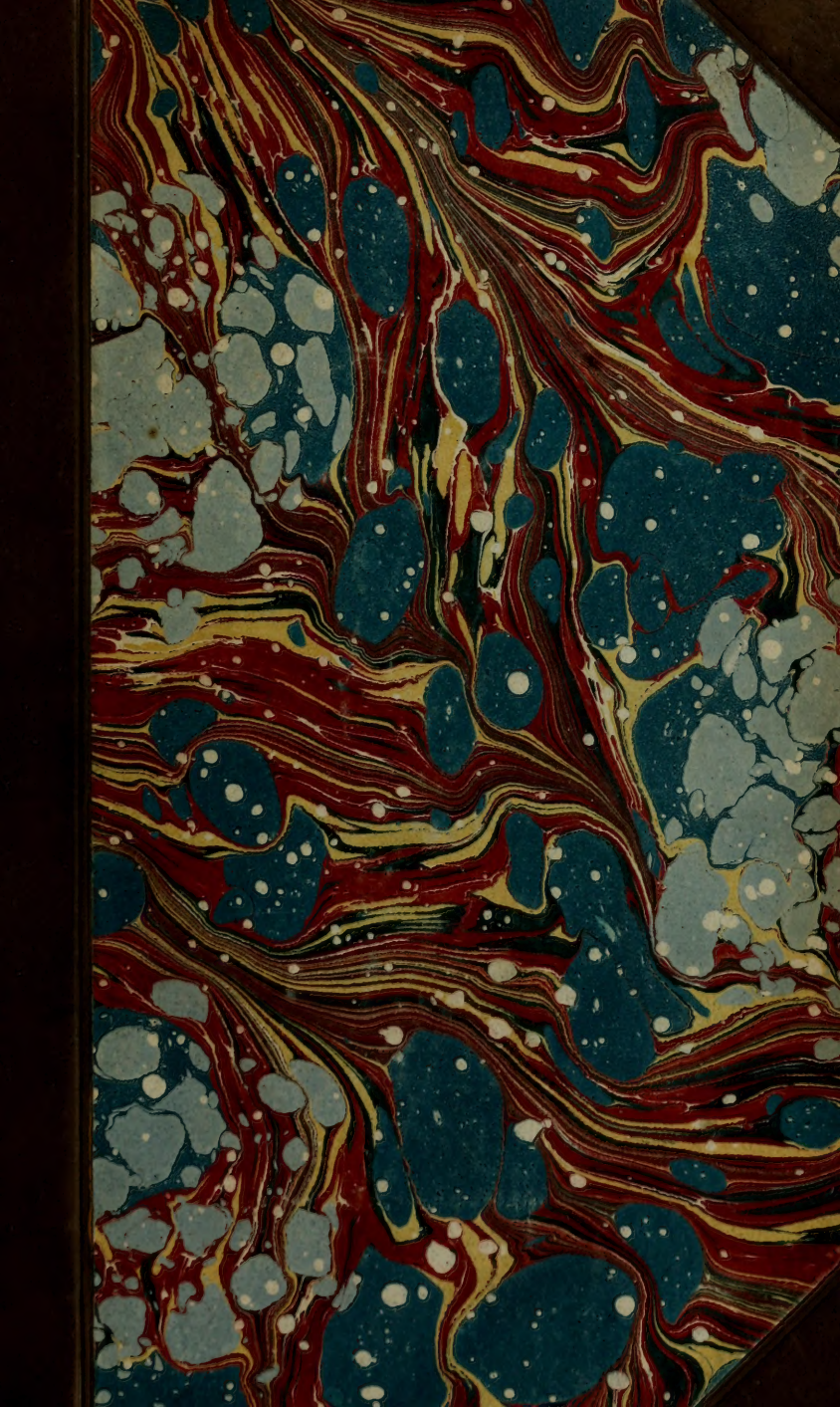
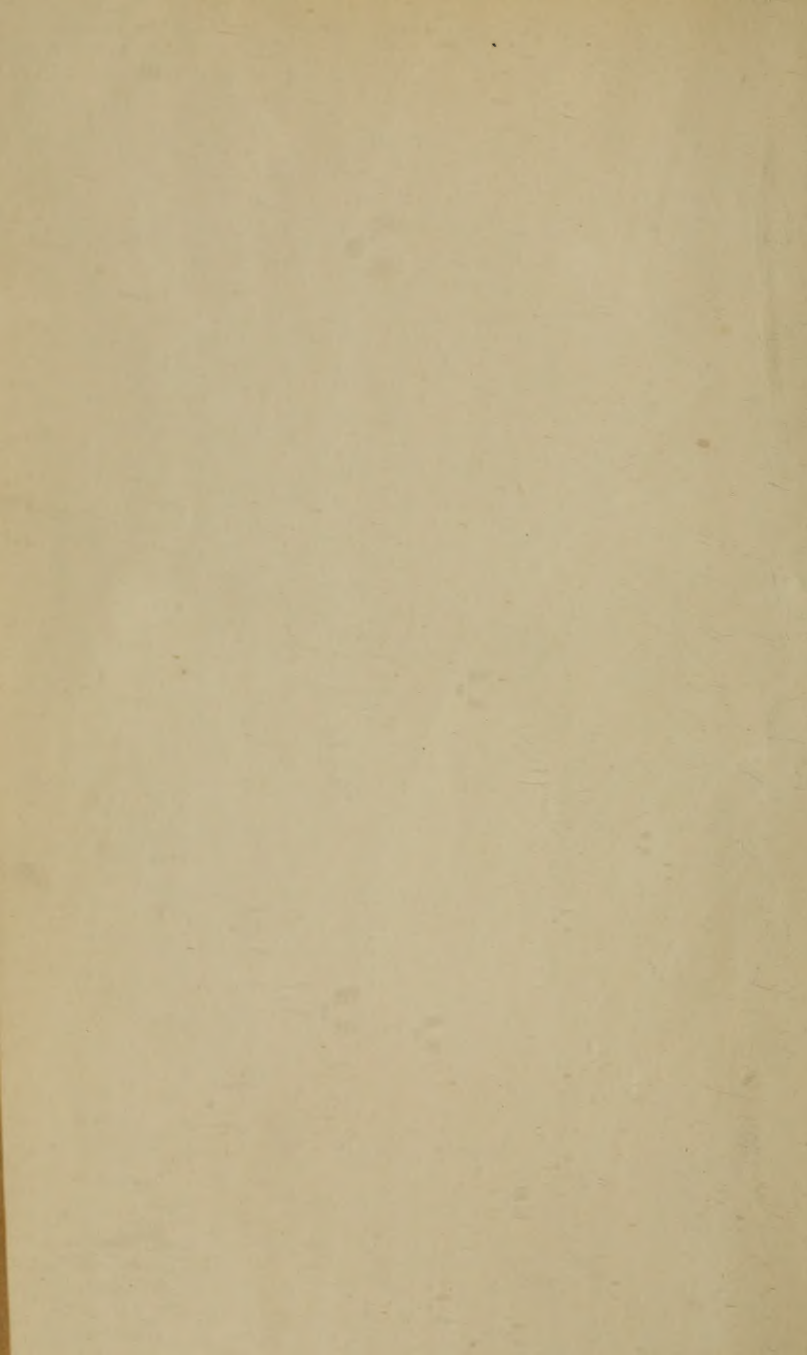
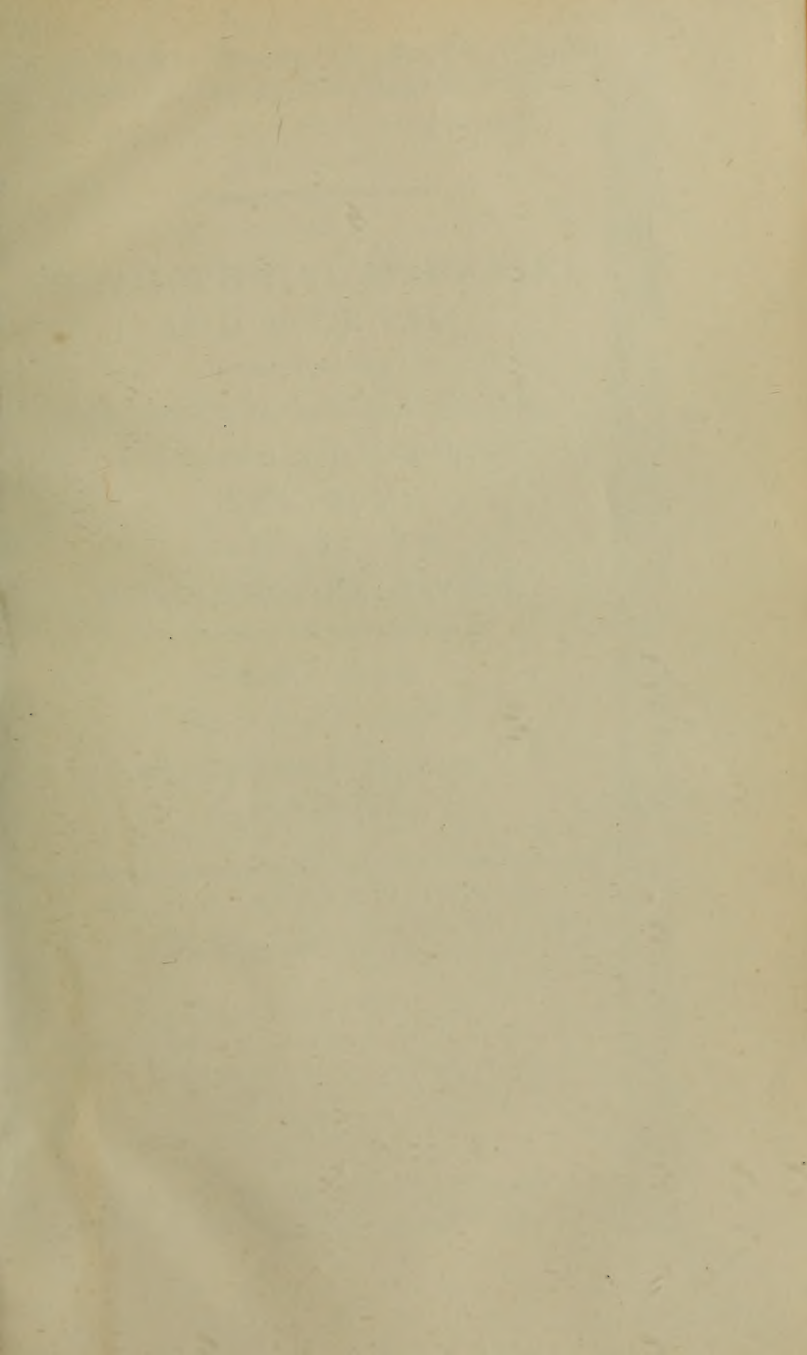




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THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL.

I. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* By
HENRY UPINGTON, Esq.

[Concluded from vol. liii. p. 253.]

To Mr. Tilloch.

Blair's Hill, Cork, May 15, 1819.

SIR, — **ALTHOUGH** I have not, in any one instance, from the commencement of these papers, specifically mentioned as the subject of investigation—whether music, abstractedly considered, was necessary to man in a state of nature; or whether the cultivation of music, to a certain extent, agreeably to that system uniformly practised in modern Europe, was necessary to the improvement of a modern orator;—yet the whole scope of my inquiry has abundantly manifested this latter as my intended topic.

Allured, during the prosecution of my design, by a variety of interesting objects, I have been insensibly conducted over a much more extensive field than the generality of my readers may possibly have desired. Not only modern but ancient melody has, in some degree, been examined. A new method of calculating the comparative concordance of our musical numbers has been suggested. The genius of our polite delivery has been ascertained by a series of experiments;—and the oratorical utility of introducing the *slide*, of acquiring the *minor third* and *fourth*, and of speaking within the limits of the *Diapente*, has been frequently represented. Our complicated *harmony*, too, has been explored; and however gratifying it may prove to the modern ear, I have particularly guarded the *Orator* against it*, as tending not only to vitiate his taste by the arrangement of its intervals, but also to create an habitual disrelish for the simpler beauties of nature, and consequently for the appropriate cultivation of the melody of *language*, upon which his influence over all classes of mankind must

* See Appendix, on *Harmony*.

so essentially depend. Even the simpler species of our modern *melody*, until considerably and judiciously limited both in time and interval, was proved by experiment to have been injurious to the Orator: neither was music, indeed, under every essayed regulation, found materially advantageous, until all previous oratorical associations were utterly effaced; for the accomplishment of which, accent, quantity, and certain exercises connected with the execution of these, were efficaciously adopted.

It is now time that we return to the *Speaker*, whom we left in our last paper systematically pursuing the improvement of his ear and enunciative organs, by a series of muscular and musical lessons arranged for the occasion, and attainable by the simplest process. A comprehensive exercise of time and forte, embracing all the simpler feet of antiquity, concluded the whole; and nothing further remained than the final application of his attainments, in the graceful and expressive delivery of those languages best calculated for this salutary purpose.

All the passages then, which I have hitherto noticed—namely, that of Virgil’s first Eclogue, of the *Æneid*, of the *Iliad*, and of Ovid’s *Metamorphoses*—those of the *Epithalamium* by Theocritus, and that of the first Philippic of Demosthenes, with the addition of *Πρῶτον μὲν* down to *βελτίῳ γένεσθαι*, were restudied and at length modulated with satisfactory chasteness and variety. At this period, the *SPEAKER* was, in a *musical* sense, so materially improved; and could retrace, with so much accuracy, his intervals—that my *ASSOCIATE* was readily enabled to note down the *Exordium* of the *Iliad*, which, almost exactly as it was spoken, has been laid before the public in *The Philosophical Magazine* of February. The only subsequent recitations in which the *SPEAKER* was exercised, were

The first stanza and part of the
second of Sappho’s well-known

fragment [as if a period] .. *Φαίνεται μοι* down to *ἰμερόεν*.

The same, in Latin, by Catullus *Ille mi* .. *dulce ridentem*.

Lord’s Prayer *Πάτερ ἡμῶν* .. to the end.

Exordium of Demosthenes

on the Peace *ΟΡΩ μὲν* .. *Προεξιμένα σωθήσεται*.

Exordium of Demosthenes

on the Crown *Πρῶτον μὲν* .. *Εἶσαι χρήσασθαι*.

Peroration on the Crown .. *Μὴ δὴτ’ ὧ πάντες θεὸς* to the end.*

And of all these, the last-mentioned peroration was infinitely the most difficult;—so difficult indeed, when spoken in quantity, that I very much doubt whether any modern organ, without previous

* “ *Patris dictum sapiens* ” was likewise executed. The result fully authorized the observation of Cicero.

discipline, can sustain the enormous burthen. Even the necessary *breath* for the just execution of this passage is almost incredible: and therefore I shall not hesitate to pronounce, that, by him who shall deliver it as he ought, the longest and most cumbrous sentence ever constructed by an English writer, shall be *commanded* with nearly as much facility as any ordinary composition.

Readings, or rather recitations in our native language succeeded those ancient passages. Milton's *Paradise Lost*, Johnson's *Rasselas*, and Bolingbroke's *Spirit of Patriotism* and *Patriot King*, were the chosen works;—and from these, several appropriate paragraphs were selected and studied. Nor was it without some trouble and perseverance that such masterly productions were, even now, delivered with becoming grace and dignity: he who would excel as an orator must enter into the true spirit of the subject which he means to read or to recite; and conceal from the most prying observer every appearance of those arts to which he has been indebted for pre-eminence. Every thing must seem natural and easy.

Apparently there is no individual topic which, with regard to the delivery of *our* language, requires more attention than that of the adjective when it precedes its substantive. Here some governing principle appeared essential to the *Speaker*,—the degradation of either word to the rank of an Enclitic,—although it may very well suit the genius of a *Goth*,—being almost in all cases subversive of expression. His own (the *Speaker's*) good sense suggested the following rule,—and practice soon familiarized the application:—That, on the first reading of a passage, the adjective be altogether omitted; and the consequence of the substantive with reference to the intent of the passage, be thus separately and independently examined, and delivered accordingly: This being accomplished, that the adjective be *then* introduced with *that species** of emphasis most indicative of its quality; the substantive retaining in the mean while, as far as possible, its originally destined character. A most useful hint to the teachers of elocution! by scarcely any one of whom have I ever heard a conjoint adjective and substantive delivered with expression. That unlucky sentence "*Will you ride to town to day?*" narrows all their conceptions: Nor, until this same and such like sentences be more scientifically understood, can we ever hope to emerge from that gulf of barbarism into which we have been plunged by our grammarians.

I proceed with my subject.—The *Speaker* having at length attained the mastery of the *sublime*, towards which his whole course

* See Magazine of April, for the true definition of Emphasis. To this may be added, that *faulty composition* will sometimes embarrass the most intelligent and expressive reader.

of discipline was evidently directed; the notation of his intervals became, in some cases, a literary desideratum. ‘The Apostles’ Creed, in which he certainly excelled, particularly attracted our attention, as furnishing us with two examples: the one, of an *ascending climax*, in decreasing *forte* or *diminuendo*; and the other, of an *ascending finish*.

The concluding portion of this Creed is sufficient for our purpose. It was *clerically* spoken, thus:

[For the method of executing this and the subsequent passages, let the reader consult the antecedent Magazines.]

I be-lieve in the Ho-ly Ghost, the ho-ly ca-tho-lic church,
the com-mu-nion of saints, the for-give-ness of sins, the
re-sur-rec-tion of the bo-dy, and the life e-ver-last-ing.

The reader will necessarily observe, that neither in the foregoing passage, nor in the passages which follow, have the rules of the *Diapente* been transgressed, except in one single instance of a semitone. Every individual *clause* throughout the whole is confined within the limits of *that* interval [the *Diapente*]; and notwithstanding the several parts of the previous climax, the modulation has been so governed, that the extent of the *Diapason* or Octave has not been reached, even from extreme to extreme.

A few words may be here offered with respect to the *forte* and *piano* of the above passage: but of this task the musician will readily feel the difficulty, and must therefore make every allowance for the inadequate description.

- “ I believe in the holy Ghost ” (Moderately loud).
 “ the holy catholic church ” (Somewhat louder and swelling).
 “ the communion of saints ” (Soft).
 “ the forgiveness of sins. ” (Very soft).
 “ the resurrection of the body ” (Louder than any preceding clause, and swelling).
 “ and the life everlasting ” (Forte and swell somewhat diminished, especially at the close).

Dryden’s memorable five *fallens* (in Alexander’s Feast), on the merits of which every petty critic thinks himself competent to decide; but which, until now, either on or off the stage, it had never been *my* lot or the lot of my ASSOCIATE to hear even tolerably

lerably recited—next claimed our attention. The theoretical notion of Lord Kaimes, that each of these words should fall progressively through the scale : the wild and indefinite jargon of Mr. Walker, whose rising and falling inflexions surpass all human understanding : but, above all, the imperfect declaration of the author of a *practical* work of some merit called “ Sheridan and Henderson’s Poems,” that the expression of these words must be varied *as well as we can*;—excited the curiosity of the SPEAKER, who, after deliberation, divided and stopped the passage thus :

“ By too severe a fate, fallen ;
Fallen, fallen, fallen :
Fallen from his high estate,
And wel’t’ring in his blood.”

He then delivered it in the following manner, within the boundary of the Diatessaron or Fourth :

By too se-vere a fate, fallen ; fallen, fallen, fallen :

Fallen from his high e-state, and wel’t’ring in his blood.

An example of the *circumflex* succeeded by a genuine *acute* must particularly interest the *scholar*, for whose gratification I shall note down the last line of Dryden’s Version of the Exordium of the *Æneid*, as recited by the SPEAKER. This is likewise confined to the Diatessaron :

And the long glo-ries of ma-jes-tic Rome.

Handel, the great master of our recitative, once more presents himself to our view. That highly extolled passage of his “ *Athalia*,” of which I have so constantly spoken, and which I have also promised in its original form,—shall be here exhibited to the reader, accompanied by an incontestable proof that *music*, composed for the specific purpose by a superior genius, must contribute to the advancement of our elocution.

This passage, having been intentionally reserved as our final subject for experiment, was not hitherto rehearsed by the SPEAKER. In the presence of my ASSOCIATE and another intelligent amateur, it was now delivered to him for recital ; was repeatedly executed by him, for our observation ; and underwent the minutest scrutiny. The music, as composed by Handel, was then taken up, played

played on the piano-forte, and sung by my ASSOCIATE. The SPEAKER followed, for half a dozen times, the example; and having possessed himself, in a suitable degree, of the spirit of the piece, he returned to the recital—and manifestly executed the passage in a style much superior to that of his original performance.

Passage, from "Athalia," as composed by HANDEL; exactly copied from Dr. Burney's History of Music.

The musical score consists of three systems, each with a vocal line (treble clef) and a piano accompaniment (bass clef). The lyrics are written below the vocal line.

System 1:
 Vocal: But as the young bar-ba-rian I ca-ress'd, he plung'd a dag-ger
 Piano: Accompaniment with sixteenth and thirty-second notes.

System 2:
 Vocal: deep with-in my breast: No ef-fort could the blow re-
 Piano: Accompaniment with sixteenth and thirty-second notes.

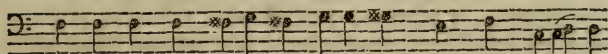
System 3:
 Vocal: pel;—I shriek'd,—I faint-ed,—and—I fell.
 Piano: Accompaniment with sixteenth and thirty-second notes.

Neither the preceding harmony, nor the relative duration of the notes was attempted by the SPEAKER,—the former being altogether out of the question, and the latter as inappropriate, in many instances, for oratory, as art could have devised. The *intervals*, therefore, were the object of the experiment; and to these only must be attributed that satisfactory result which I have just represented.

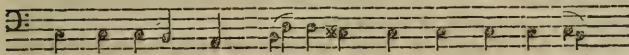
How far the genius of the original has been carried into language will appear from the subjoined copy of the *recitation*, as noted down by my ASSOCIATE. The situation of "*dagger*" with respect to "*plunged*," as expressed by Handel, indicates theatrical rant—not the chaste delivery of an orator; but whether the *finish*, as executed by the SPEAKER, is or is not superior to that of the original if applied to speech, the musical world may decide. It virtually contains but *two ultimately falling syllables*, in consequence of the pause or rest by which the antepenultimate word "*and*" is succeeded.

INTER-

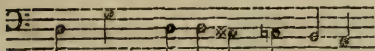
INTERVALS of "*Athalia*," as recited by the SPEAKER.



But as the young bar-ba-rian I ca-ress'd,—he plunged a dag-ger



deep with-in my breast: No ef-fort could the blow re-pel;—



I shriek'd,—I faint-ed—and—I fell.

I have now brought my labours to a conclusion; sustained, throughout the progress of my inquiry, by the well-grounded expectation that a subject of this nature, from its intimate connexion with the very structure as well as the delivery of speech, must influence the literature of my country. May, then, the learned guardians of our seminaries employ those faculties with which they are so eminently gifted, for the improvement of our native tongue: and may they never cease to remember—that, although we have excelled our late gigantic adversary both in arms and in arts—HIS LANGUAGE IS YET TRIUMPHANT. In all political negotiations it continues to supersede our own.

HENRY UPINGTON.

Appendix on Harmony

(to which I have referred in the preceding paper).

In my former papers on this subject, I have principally addressed myself to the philosopher and mathematician: In this place, then, I shall observe to the musician, that, unless I have taken a very imperfect survey of the question, our present harmonical edifice is founded upon so many contradictory propositions, that, view it in what manner we please—whether philosophically, mathematically, logically, or even *naturally* as we term it,—it is altogether indefensible. Some half a dozen of the most prominent of these propositions, with their deductions, will serve as an example of the whole.

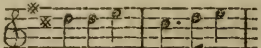
PROP. 1st. That 2, 4, 8, 16, &c. even to infinity, are equal to one; or, in other words, that octaves are equal to unisons:

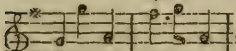
And therefore—that, since Nature accompanies every individual sound by the upper octave of its fifth, and the upper double octave of its major third; the *immediate* third and fifth of the original sound may be substituted in their stead.

And—that, although in our *fundamental base* experiments
(agreeably

(agreeably to Rousseau’s table, article “System,”) every given note and its major third, as C, E = 15, 12, produce, when struck in conjunction, a *third* sound = 30 called the lower octave of that given note C; we nevertheless decide that that lower octave or *thirty* is the original note or *fifteen* itself;—and are consequently enabled, by this wonderfully rational metamorphose, to prove the infallibility of our BASE.

PROP. 2d. That 2, 4, 8, 16, &c. are *not* equal to one: for if, in the two first bars of “God save the King,” for example, the

original notes  were represented by

, we could not recognise the tune at

all. Wherefore the substitution of octave for unison is inadmissible; and must, if admitted, expose us to the fate of our harmonic founder Rameau, who, on advancing the singular proposition that *one* is equal to *two*, was laughed at by all the world. “Tout le monde s’est moqué de lui,” says Rousseau, “and the Academy openly disapproved.”

PROP. 3d. That of four strings called our *common chord* = 15, 12, 10, $7\frac{1}{2}$; the upper string or $7\frac{1}{2}$ being *absorbed* by the lower string 15, is equal to *nothing*, and may be retrenched.

And therefore—that *harmonic proportion* does actually exist within this common chord, as indicated by its *three first* strings, independently of its *fourth*. So says Tartini.

PROP. 4th. That of the foregoing strings = 15, 12, 10, $7\frac{1}{2}$; the upper string or $7\frac{1}{2}$ is *not* absorbed by the lower string 15,

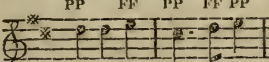
And therefore, *not* being equal to *nothing*, cannot be retrenched. On the contrary, it imparts so decidedly novel a character to the whole; that, without it we should have only an inconclusive group incapable of effectuating *any sort of finish*.

PROP. 5th. That *strong* and *weak*, called Forte and Piano, are equal.

And therefore—whether in the case of a single string accompanied by its 12th and 17th [upper octave of its 5th, and double octave of its 3d]; or in the case of that *third* sound called *fundamental base*, engendered by two coexisting sounds: although Nature produces all such accompanying or engendered sounds in comparative *piano*, and more generally in *pianissimo*—yet that these sounds must be brought forward in *forte*.

PROP. 6th. That *strong* and *weak*, called Forte and Piano, are *not* equal.

And therefore must not be confounded; otherwise we should have much worse than no music at all. “God save the King,” if

if executed thus  even by Rameau and Tartini themselves, would instantly upset their reputation.

PROP. 7th. That the audible notes or *satellites*, viz. the upper 12th and 17th major, attendant upon every existing note or sound, are *Nature's own harmonics*, or *conords*.

Wherefore the 12th and 17th of every original note employed may be introduced into our music—as exemplified by our larger organs.

PROP. 8th. That the satellites or 12ths and 17ths major of every original note, though called *Nature's own harmonics*, are *not* CONCORDS, and *cannot* be introduced; as may be exemplified by violins and a violoncello.

And therefore—if in any piece whatsoever, the said 12ths and 17ths were uniformly set down, and the piece arranged for these instruments so as to represent the compound stops of an organ,—each violin, as well as the violoncello, playing with equal loudness—and duplicates of no one note being permitted for the purpose of *increasing the noise* of that note,—we should scare the Hottentots.

Here end these few propositions, which, although seemingly contradictory, our well informed musicians will, no doubt, reconcile to our understanding. But how happens it that any piece of music horrific on the violin and violoncello should be tolerated on the organ? The answer is easy. A little *salutary* NOISE can always be obtained upon the organ, through a variety of roaring pipes called *stops*, to “*soften and conceal*” our discordant conords. [*Soften and conceal* are the very argumentative terms of our *Encyclopædia Britannica*, article “HARMONY.”] And indeed, if in the above case of our simple violins and violoncello, a sufficient number of drums, trumpets, bugles, and French-horns be introduced—not delicately to *soften and conceal*, but vociferously to *roar down* those feeble instruments,—we shall immediately and miraculously convert an otherwise intolerable jargon into the most exquisite and scientific harmony!!!

II. Report by the Select Committee of the House of Commons, on Mr. TELFORD's Plan for building a Hanging Iron Bridge across the Menai Strait*.

YOUR Committee have proceeded to inquire into the subject of the papers referred to them by the House, containing Mr. Tel-

* From the Third Report from the Select Committee on the Road from London to Holyhead, &c. 29th April 1819.

ford's plan for building a hanging iron bridge across the Menai Strait, and the evidence taken last year by the Holyhead Road Commissioners.

With the view of being able to lay before the House all that could be advanced to prove the practicability of this plan, your Committee examined Mr. Telford concerning each specific part of it, and they then examined Mr. Rennie, to ascertain how far he concurred in the calculations and opinions of Mr. Telford: they also examined Mr. Donkin, whose attention has been for a considerable time applied to this principle of bridge-building, and who ranks very high as a civil and practical engineer, and is Chairman of the Committee of Mechanics of the Society of Arts and Manufactures.

In order to bring the whole subject before the House in the most distinct manner, it appears to be advisable to treat of it under the following heads:—1. The Abutments.—2. The Iron Work.—3. The Strength of the Bridge.—4. What probable Undulation or Side Vibration.—5. What Contraction or Expansion.—6. The Means of Repairing the Bridge.

1. *The Abutments.*—The abutments will consist of the whole of the masonry work which is expressed on the plan; each of the two principal piers will be 60 by $42\frac{1}{2}$ feet at high-water mark, having a foundation of rock. These piers when connected with the whole of the remainder of the masonry will form a mass constructed with blocks of hard limestone, of much greater power than is requisite for supporting a bridge of this kind. Mr. Rennie being asked this question, "Can there be any difficulty in making the piers capable of bearing the bridge?" answered, "None in the world;" and explained to your Committee, that it was equally practicable to make a pier to sustain a weight drawing inward, as this bridge will draw, as to sustain a weight pressing outward, in the way an arched bridge presses: from thence he argued, that the lateral tension of the proposed bridge would not occasion any difficulty. He mentioned that the lateral pressure of the side arches of the Southwark bridge was about 3,700 tons, and that this was infinitely greater than the strain of Mr. Telford's bridge.

Upon the summit of each of the two main piers will be erected a frame of cast metal, of a pyramidal form, for the purpose of raising the cables, from which the bridge is to be suspended. As the cables will be carried from the tops of the pyramids, so as to form nearly similar angles on each side of them, the pressure will be almost perpendicular; and Mr. Telford says, "It is quite impossible the weight of the bridge can crush them, in consequence of the well ascertained fact, that it requires a weight of from four to five tons to crush a cube of a quarter of an inch of good cast metal."

2. *The*

2. *The Iron Work.* — Mr. Telford proposes to have four lines of suspension in the breadth of the bridge, by which means his cables will be disposed in such a manner, as to divide it into two carriage-ways of twelve feet each, and one footway in the centre of four feet. Along each line he will have four cables, making in the whole sixteen; these cables will pass over rollers fixed on the summits of the pyramids, and be fastened at their extremities to an iron frame, lying horizontally over the tops of the small arches, and under a mass of masonry, as described by the dotted lines on the plan. (Plate.)

From these cables the roadway will be suspended by vertical iron rods, connected at their lower extremities with wrought iron bars, both transversely and longitudinally, thus forming a frame on which timber will be laid for the roadway.

Mr. Telford intends to make a temporary wire bridge from one abutment to the other, in order to carry over the cables, and arrange the several parts of the bridge.

3. *The Strength of the Bridge.* — Mr. Telford informed your Committee, that for many years past he has bestowed great pains and considerable expense on making experiments for the purpose of ascertaining the strength of malleable iron: he says, in his evidence, that he has not made less than 300 experiments upon iron, from one-twentieth of an inch to two inches in diameter, and from 30 feet in length to 900 feet; that he has made them vertically, horizontally, and with a variety of different degrees of curvature; that he has combined iron into the shape in which he has proposed to make the bridge, in a model of 50 feet in length, and tried experiments upon it. That the greatest part of his experiments were made from absolute weight, by tearing iron to pieces by mere weight, and that all his calculations were founded upon the true and actual strength of iron, as proved by the weight it would sustain before it would stretch or break asunder. In respect of the larger pieces of iron, which could not conveniently be torn asunder by weight only, Mr. Telford made several experiments upon the strength of it, by means of an hydraulic press of Mr. Brunton's, made on Mr. Bramah's principle; and by this it appeared, that it required a weight of from 26 to 30 tons to tear a square inch bar asunder: similar experiments have been tried with other machines, which gave very nearly the same result. Mr. Rennie and Mr. Donkin, in their evidence, entirely agree with Mr. Telford's statement of the absolute strength of malleable iron. Mr. Rennie recommends, that this bridge should be constructed, so as to be four times beyond the strength requisite to carry its own weight; that is, to make it something stronger than it is proposed to be made by Mr. Telford; but Mr. Telford says, there will be no difficulty in giving

giving it such an additional degree of strength, for by increasing the quantity of iron, you may gain any additional power.

Mr. Telford submitted the whole of his experiments to the examination of Mr. Barlow, who is the Mathematical Master at Woolwich Academy, and who has published the greater part of them in his work on timber and iron. Mr. Barlow states, that the theoretical calculations which he has made, correspond with those which are deduced from practical experiments.

It appears by Mr. Telford's evidence, that the weight his cables will support before they will break is 2,016 tons, exclusive of their own weight: the weight of the bridge, exclusive of the cables, is 342 tons; therefore, the bridge will bear 1,674 tons beyond its own weight.

Mr. Donkin, on being asked whether it appeared to him that the different calculations of strength made by Mr. Telford were accurate? replied, "Mr. Telford seems to have taken his primary data from the fact of a bar of iron, -one inch square, beginning to stretch at half its absolute power; in all the experiments that I have witnessed of straining iron, I think none of the bars began to stretch permanently under nearly two-thirds of it; it appears to me, therefore, that Mr. Telford's data are perfectly safe."

4. *Undulation and Side Vibration.*—Mr. Telford says, there is not much reason to expect undulation from any weight being laid on any particular part of the bridge, in consequence of so great a weight as 489 tons (the weight of the whole bridge) hanging between the points of suspension; but to guard against it, he proposes to make the four sides of the roadways of framed iron-work, to be firmly bound together for seven feet in height, and to have similar work for five feet in depth below the cables; so that when they meet towards the middle of the bridge, they will constitute a frame-work of twelve feet deep on each of the roadways, which will also form a complete protection to passengers. In respect to side vibration, Mr. Telford says, the proportion which the breadth of the bridge bears to the length of it, will keep it quite steady.

Mr. Rennie says, "This bridge being to be covered with timber, it makes a single plank of 522 feet long and 30 feet wide; and I conceive that the shock can scarcely be any thing sideways." And on his being asked by the Holyhead Road Commissioners, "What effect do you apprehend the wind would have upon a bridge of this construction?" answered, "My opinion is, that from the strength of the iron and the weight of the bridge taken together, there would be no injury in that way."

5. *Contraction and Expansion.*—On these heads Mr. Telford and Mr. Rennie calculate, there may be a rise or fall to the extent

tent of four or five inches; but both agree, that the changes arising from the temperature will not derange the bridge.

6. *Repairs.*—It appears from the evidence, that the cables, suspending rods, and roadways, will be so constructed and united together, that each part may be taken out and repaired separately.

Your Committee feel great satisfaction in having it in their power to say, that Mr. Telford has completely convinced them of the practicability of his plan. The numerous instances which he has already given to the public of his talents as a civil engineer, fully prove that the House may place great confidence in his opinion. But when his opinion is supported by Mr. Rennie and other engineers, the case of the practicability of this undertaking appears to be as completely made out as it is in the nature of things to allow of its being established.

When it is remembered, that the first estimate prepared for Lord Colchester (when Chief Secretary for Ireland) for building a cast iron arched bridge across the Menai amounted to 268,500*l.* your Committee are of opinion, that the public stand greatly indebted to the industry and talents of Mr. Telford, for having contrived a plan on so secure a principle, for executing this work for the sum of 70,000*l.*

April 29th, 1819:

The following particulars are copied from the Appendix to the foregoing Report.

Mr. Telford in answer to a question by the Committee gave in the following statement in writing:

“In order to avoid interrupting the navigation, it is evident that a horizontal roadway is most advisable; and to obtain this, a bridge, upon the principle of suspension, seems unavoidable; it is therefore adopted at the height of 100 feet above the high water of spring tides. The distance between the points of suspension is 560 feet, and the versed sine is 37 feet or about 1-15th of the chord line. The breadth of the bridge will be about 30 feet, having two carriage-ways of 12 feet each, and a footpath of four feet between them. This affords four points of suspension in the breadth of the bridge. The whole roadways are to be suspended from the main cables by means of perpendicular rods, and are therefore to be considered as mere weight. The iron-work of the cables and timber of the roadways are to be constructed so that they may be taken out and replaced separately.

“By calculation I find that the weight to be suspended is 342 tons: by numerous experiments which I have made to ascertain the strength of malleable iron, it appears, that with a chord line of 560 feet, and a versed sine of 37 (or a curvature of 1-15th), a bar of good iron, one inch square, will, besides its own weight, carry

carry $10\frac{1}{2}$ tons, and about one half of that weight before it begins to stretch. For the Menai bridge, I have taken a section of 192 square inches, which at $5\frac{1}{4}$ tons to each square inch, will support 1,008 tons, being a surplus of 666 tons above the real weight of the bridge, and there would be required a further weight of 1,008 tons to break down the bridge: this I conceive is making ample provision against any probable trial to which such a bridge can be exposed. From the elevation it will be seen, that the cables attain their curvature by passing over cast-iron frames, part of which are of a pyramidal form, and the other parts are connected with the top of the masonry; from thence it will be seen, by dotted lines, that these cables pass down the masonry to another cast-iron frame, laid horizontally along the top of the arches, and connected with their springers by means of perpendicular rods, thereby embracing the whole mass of masonry and spandrels, making in all about 12,000 tons at each end of the bridge, and this exclusive of the great pyramids. As the weight of the bridge between the two points of suspension, including the cables, is 489 tons, there is not much reason to expect undulation from any weight which will be laid on any particular part; but to guard against any effect of that sort, I propose making the four sides of the roadways of framed iron-work firmly bound together for seven feet in height, and similar work for five feet in depth below the cables, which when they meet towards the middle of the bridge will constitute a frame-work of twelve feet deep.

“With a bridge 30 feet in breadth, and 532 feet in length, there is not much to be apprehended from side vibration; but in order to provide against this operation, I have in the plan placed two horizontal cables, crossing the bridge diagonally: each laying hold of the middle of its length, and passing round a cast iron projecting frame, at the opposite sides of the great pyramids, is from thence carried to the masonry of the abutments; thus creating a diagonal stay upon 70 feet in breadth.

“When it is considered that from four to five tons are required to crush a cube of one quarter of an inch of good cast-iron, there can be no doubt of the sufficiency of the cast-iron frames over which the cables will pass.

“These cables are continued to the cast-iron frames which connect the masonry of the abutments. The weight of the bridge is 489 tons, upon which if 300 tons additional are placed, they make 789 tons. The pull of this weight at the abutments, upon a curvature of one-fifteenth, is found by my experiments over a pulley, with a perpendicular weight, equal to about two and a half times the weight on the other side, or 1,972 tons. To counteract this, the cables are, as has already been observed,
continued

DESIGN FOR A BRIDGE over the MENAI STRAITS at YNSY-MOCH.

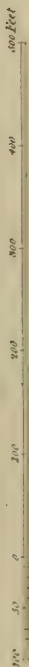
By Thomas Telford, Esq., &c.

See Plan

High Water Line
1 in 11 1/2 ft. from

County of Carnarvon

County of Anglesea





continued at nearly the same angles as those of the bridge, to the cast-iron frame, which embraces about 12,000 tons of masonry, and to which much more if necessary might be connected.

“ With regard to any change by contraction or expansion, it is known from experiments, that with a difference of temperature of 90 degrees of Fahrenheit, the difference of length of iron would only be $\frac{6}{100000}$, or about five inches upon 700 feet ; and as the iron-work would most likely be put up at a mean temperature, the contraction would be two and a half inches, and the expansion an equal quantity, which would not derange the bridge ; but if the main suspending cables were covered with some substance, which was an imperfect conductor of heat, and which is intended, the above variation of 90 degrees of temperature could not take place.

“ I have thus, for the satisfaction of the Committee, stated the principal circumstances relative to this plan, and which have induced me to recommend it. The numerous and tedious details which are connected with such a work, I presume the Committee do not expect me to go into here ; and as the having a suitable and substantial bridge is the sole object in view, I shall most thankfully receive any useful improvements that may be suggested by others. (Signed) THOMAS TELFORD.

“ Dated London, 23d April 1819.”

Mr. Rennie, on being asked if he had seen any instances of bridges upon this construction of a large size ? answered, No, I have not ; the only thing of that kind I have seen is a model that was made by Captain Brown, who is an iron cable manufacturer at Mill Wall, Isle of Dogs, of a bridge nearly of this construction, of 120 feet span, and over which I was drawn in a carriage, and found myself perfectly safe and easy.

Where was that bridge erected ?—At Captain Brown’s manufactory, Mill Wall, Isle of Dogs, on the land, merely as an experiment ; and I suppose it is standing yet, where I believe it may be seen at this day ; I came out of the carriage, and made the coachman drive several times over it, that I might see how it acted.

There was no vibration ?—Very little vibration.

Can you calculate the weight that would be distributed on such a bridge as this of Mr. Telford’s, supposing it was filled as full as might be with a drove of oxen ?—I cannot answer that question off hand.

[*To Mr. Telford.*]—Can you answer that question ?—The weight, as far as I can calculate, of covering it with oxen will be about three hundred tons, it depends on the weight of the cattle ;

18 *Plan for building an Iron Bridge across the Menai Strait.*

but in the usual way of driving cattle, there is never that quantity together, that is, supposing the whole bridge covered from end to end, and no void space left, which is a very unusual thing. In driving cattle, nobody ever thinks of driving 200 head all in a heap.

[*To Mr. Rennie.*]—What power of resistance beyond the probable weight that could be put upon such a bridge, by cattle or otherwise, should you conceive to be necessary to bear up a bridge of this description?—I should not think it secure unless it was capable of carrying at least four times its own weight; for instance, suppose the carrying itself was 300 tons, I should make it capable of carrying four times that weight at least, in addition to its own, that is, 1500 tons; but this proportion would not hold if it was a light bridge. If you will allow me to state with respect to lateral pressure, or rather lateral tension, I should think there is no difficulty in that respect; for in the bridge I have constructed over the 'Thames, at Queen-street, the lateral pressure of the side arches is about 3700 tons, which is infinitely greater than any thing that would ever be wanted here.

That being an arched bridge, how does the comparison hold?—The pressure of an arched bridge is outward, the draft of the other is inward, and I conceive it equally practicable to make a pier to sustain the weight inward as outward; and in the experiments I have made, I have found that the force necessary to crush the materials against which the iron acts, is between twenty and thirty times the lateral pressure. I made a set of experiments to ascertain the fact as to the particular kind of stone used in the abutments and piers.

Did you ever see the chain bridge over the Tees?—I never saw it. When the Bell Rock light-house was building, which is about ten miles out at sea, a smith's forge and other matters were erected on a different part of the rock to which the light-house was; and, in order to have a communication for the heavy materials from the forge to the light-house, we had one of those bridges, which answered very well.

What length was it?—Fifty or sixty feet; I do not know exactly, but it was full that, I know. I would observe, that this bridge being to be covered with either planks or wood, it makes a single plank 522 feet long and 30 feet wide, from which I conceive that the shake can scarcely be anything sideways.

Do you entertain any doubts with respect to the practicability of constructing such a bridge as that which is proposed by Mr. Telford, according to the plan which he has just stated to the Committee?—I have no doubt of the practicability of constructing such a bridge; and I presume that Mr. Telford has taken
care

care to have all the parts sufficiently strong, and so well connected together, as to be able to sustain the weight he has calculated it to bear; but I have not made any calculations of the actual strength of the bridge which Mr. Telford proposes, though I am satisfied that a bridge of that construction may be made sufficiently strong for the purpose.

Can there be any difficulty in making the piers capable of bearing the bridge?—None in the world.

Does not the whole depend merely upon a matter of calculation?—Certainly, and the judgement of the person who puts it together.

You have made no calculation as to the weight and bearing of the bridge?—I never have, because I never saw the plan until it was just now produced before me.

[*To Mr. Telford.*].—What greater power of resistance than the actual weight of the bridge itself have you provided for in your calculation?—Rather more than four times the power. The whole of the bridge is 489 tons, and the power of suspension I calculate equal to 2016 tons.

What is the sectional area of your cables?—I have given the relative power to the weight; it is more than four times.

State the sections of your cables?—The section of the cable (taking it as a single cable) is 192 square inches.

[*To Mr. Rennie.*].—Will a bridge, constructed upon these data, be sufficient to bear a power of resistance four times greater than its own weight?—I should think it would; but if it was my own case, I should make it a little more, that is to say, I should make it four times beyond the strength requisite to carry its own weight.

[*To Mr. Telford.*].—Would there be any difficulty in giving this bridge that additional strength?—None at all; only the additional expense of that quantity of iron; you may have any quantity of iron which will give the proportional power.

III. *On the Nature and Laws of Friction: being a Continuation of the Paper on the same Subject, given in our last Volume**. By Mr. THOMAS TREDGOLD.

On the Friction of Rolling Bodies.

ONE of the most simple, and at the same time one of the most important applications of the rolling motion, is that of wheel-

* See vol. liii. p. 8.

carriages :—and to the nature of the resistance which such carriages would experience on an uniform plane, I propose to confine my inquiry. The effect of small eminences and asperities, and other circumstances of an accidental nature, has often been considered ; but that kind of resistance which is constant has not, that I am aware of, been attempted on any sound principles.

Accidental obstructions may be rendered less frequent, or in a great measure removed, by improving the art of road-making ; but there is a resistance which wheel-carriages experience, that is independent of these obstructions, and which will be only more uniform in its action the better roads are made. It is from the investigation of this kind of resistance only, that maxims for the construction of wheel-carriages can be drawn ; consequently, the importance of any attempt to investigate it must be evident, whatever may be the result.

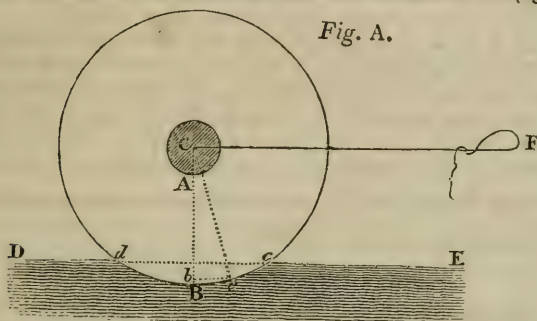
When a physical problem is of a complicated nature, it is sometimes necessary, and at other times convenient, to simplify the operation. This may be done in two ways. The one consists in neglecting certain physical circumstances, of which we have numerous examples in works on Mechanics : thus, in the investigation of the mechanical powers the effect of friction is neglected ; yet it is impossible that a mechanical power can act without friction :—and various other instances might be noticed. The other way consists in neglecting certain quantities that are very small when compared with the principal quantities in the equations. This is perhaps the most certain of the two methods ; because it is easier to form some notion of the extent of the error introduced by it than by the preceding one ; though both have sometimes led to very erroneous conclusions. But it is so great an advantage to express the laws of resistance in simple terms, that even a distant approximation to truth is fully compensated by it.

A.—When a rolling body, such as the wheel of a carriage, moves upon an uniform horizontal plane, the resistance which opposes its motion is of two kinds.

For a wheel is retarded by the rubbing of the parts at the axis, which is properly called friction ; and it is also retarded at the circumference by that part of the plane immediately before the wheel ; as that part must be depressed to the same level as that on which the wheel had previously borne, otherwise the wheel must be supposed to move over the plane without making an impression proportional to its weight and time of action, which would be absurd.

Of that Part of the Resistance of a Wheel-carriage that arises from Friction at the Axis.

B.—If a wheel, of which the radius of the axis is CA (fig. A)



and the radius of the wheel CB, be put in motion over a horizontal plane DE, and the wheel slides; then, the friction at the axis multiplied into the radius of the axis, is greater than the friction at the circumference of the wheel multiplied into its radius.

For the weight of the load acts in a vertical direction; and when the wheel is devoid of friction both at the axis and circumference, a very small force in the horizontal direction CF would cause it to roll forward. Again; suppose there to be friction at the axis only, then the horizontal force would cause the wheel to slide in the same manner as if the wheel and axis were one body; because in that case friction at the circumference would be necessary to destroy the effect of the friction at the axis, and to cause the wheel to move round it. Now consider C as the centre of motion; then the friction at the circumference acts with a leverage equal to the radius of the wheel, that at the axis with a leverage equal to the radius of the axis. Let R be the radius of the wheel, and F the friction at the circumference, r the radius of the axis, and f the friction of the axis. By the property of the lever, we have $R \times F = r \times f$ in the case of equilibrium. And when $R \times F$ is less than $r \times f$, the difference places the wheel in the same state as when there is a friction equal to that difference at the axis only, consequently the wheel would slide.

Cor.—Hence it appears that in practical cases a wheel will always roll, as the conditions necessary to produce a sliding motion are scarcely within the limits of possibility. For if the rubbing surfaces at the circumference had only the friction of polished glass, while those at the axis had the friction of wood, the wheel would roll when the radius of the wheel exceeded three times the radius of the axis.

C.—The effect of the friction at the axis in retarding the motion of the wheel will be expressed by $\frac{r \times n \times W}{R \times v}$: where W is the weight of the load, v the velocity, and n the ratio of the friction to the pressure corresponding to some given velocity.

Let a denote that part of the moving power which is employed in overcoming the friction at the axis. The friction at the axis acts with the leverage r in retarding the motion, and the part a of the power acts with the leverage R : that is, in the case of equilibrium, $R \times a = r \times$ friction at the axis. And because the motion of wheel-carriages is sensibly uniform, the friction is as $\frac{W}{v}$ *; or friction $= \frac{n \times W}{v}$, where n is a constant to be determined by experiment. Hence we have

$$R \times a = \frac{n \times r \times W}{v} \text{ or } a = \frac{n \times r \times W}{R \times v}.$$

The effect of the resistance at the axis is directly as the radius of the axis, and inversely as the radius of the wheel: therefore, when the resistance at the axis only is considered, it is an advantage to make the radius of the wheel as large, and the radius of the axis as small as possible. Also, the greater the velocity the less the friction.

Of that Part of the Resistance of a Wheel-Carriage which arises from the Action of the Circumference upon the Road.

It may be assumed that the external rim of the wheel is very hard in respect to the road it moves upon, and that the load will, in all cases, cause it to sink in the road.

D.—When a wheel rests upon a horizontal plane, the depth of the impression will be nearly expressed by $\frac{W \times q}{b \times y}$; where W is the weight of the wheel and load; b the breadth of the wheel; y the ordinate of which the corresponding absciss is equal to the depth of the impression; and q a constant quantity to be determined for each particular road by experiment.

For the ultimate depth of impression is directly as the force, and inversely as the area the force acts upon; but the area is as $y \times b$, and the force is as W : therefore denoting the depth of the impression by x ; $x : \frac{W}{y \times b}$, or $x = \frac{W \times q}{y \times b}$. Otherwise, the quantity of matter displaced is proportional to the force which displaces it. The area of the segment dcB , (fig. A.) multiplied into the breadth of the wheel, will express the quantity of matter displaced: and as the depth of the impression is always very small compared with the radius of the wheel, the area dcB will

* See Phil. Mag. vol. liii. p. 6.

be sensibly proportional to $y \times x^*$. Hence $y \times x \times b$ is as W , or $x : \frac{W}{y \times b}$, as before.

E.—When a wheel moves forward upon a horizontal plane with any velocity v , the depth of the impression will be expressed by $\frac{W \times q}{y \times b \times v^2}$.

The depth of the impression is as the force, and as the square of the time the force acts; but the time is inversely as the velocity of the wheel: therefore $x : \frac{W}{v^2} \uparrow$; which combined with the effect of the area gives $x = \frac{W \times q}{y \times b \times v^2}$.

As the resistance which the part of the road immediately before the wheel offers to the motion, arises from the pressure against every point of the arc Bc ; the sum of these pressures may be considered as collected in one point of the arc, which point may be called the centre of resistance.

F.—The centre of resistance will be nearly at the horizontal distance $\frac{1}{2}y$ from the perpendicular CB .

For the resistance at any point is proportional to the depth of the impression at that point; and, assuming that the arc Bc does not sensibly differ from a straight line, it may be considered proportional to the distance from c , and consequently to be collected at the horizontal distance $\frac{1}{2}y$ from BC .

G.—At any instant of the motion of a wheel upon a horizontal plane, the load W is to that part of the power which overcomes the resistance at the circumference, as the radius is to the tangent of the arc Be ; e being the centre of resistance.

The lines Cb , be and Ce , (fig. A.) are respectively parallel to the directions of the weight, power, and resistance; and therefore constitute a triangle of which the sides are proportional to these forces. But $Cb : be :: \text{radius} : \tan. Be$; therefore

$R : \tan. Be :: W : p$; where p is that part of the power which is employed in destroying the resistance.

In general, the value of x will be very small compared with the other quantities: therefore to render the expressions less complicated, the relation between x and y may be expressed by $y^2 = 2Rx$, and the tangent of the arc Be may be supposed to be equal to $\frac{1}{2}y$.

Substituting for x in the equation $x = \frac{W \times q}{y \times b \times v^2}$, (Prop. E.); and making $\tan. Be = \frac{1}{2}y$ in the equation $\frac{W \times \tan. Be}{2R}$, (Prop. F.), we

* See Emerson's Fluxions, p. 260, Ex. 17: 2d edition.

† Phil. Mag. vol. liii. p. 6. Prop. (5.)

have $p = \frac{W^{\frac{4}{3}} \times q^{\frac{1}{3}}}{1.5874 R^{\frac{2}{3}} \times b^{\frac{1}{3}} \times v^{\frac{2}{3}}} =$ the power that would overcome the resistance at the circumference.

And, $a + p = \frac{n \times r \times W}{R \times v} + \frac{W^{\frac{4}{3}} \times q^{\frac{1}{3}}}{1.5874 R^{\frac{2}{3}} \times b^{\frac{1}{3}} \times v^{\frac{2}{3}}} =$ the force or power that would keep the wheel in motion.

The value of the constant n may be derived from experiments on the friction of the axis ; and when the value of n is previously determined, the value of q might be obtained from experiments on wheel-carriages. But as q will differ according to the nature of the road, its precise value is not of much importance ; and particularly where a rigid analysis of the subject has not been attempted. All I have endeavoured to do, is to show an useful approximation to the laws of resistance according to the radius, the breadth, and the velocity of the wheel, the radius of the axis, and the weight of the load.

Comparing the resistance at the axis, $\frac{n \times r \times W}{R \times v}$, (Prop. C.), with the resistance at the circumference $\frac{W \times \tan. Be}{2R}$, (Prop. G.),

it appears that these resistances will be equal when $\frac{n \times r}{v} = \frac{\tan. Be}{2}$. Now, if we consider $\frac{n}{v}$ to be equal .25, which will be nearly the true value of the friction in wheel-carriages, then $\tan. Be = .5r$. In general the depth of the impression will be greater than corresponds with this value of the tang. of Be ; and whenever it is greater, the resistance at the circumference will exceed the resistance at the axis. It would be desirable that some observations should be made on this subject, as then we could better compare the effects to be gained by any variations in the construction of wheels.

It must be remembered, that in estimating the resistance at the circumference, the surface of the road has been supposed to be an uniform horizontal plane ; but granting the road to be horizontal, it will in practical cases be irregular, or covered with small asperities. Now, the effect of these asperities will bear some proportion to the number of them that the wheel is raised over in a given time, which will depend on the breadth of the wheel; consequently a broad wheel will increase instead of lessen the friction, unless the road be perfectly smooth.

From the preceding investigation the following conclusions are drawn : but it may be necessary to remind the reader that they apply to such roads only as are nearly horizontal.

1. The resistance at the axis is directly as the weight of the load. That is, if the load be doubled, the resistance at the axis will be doubled ; and so on.

II. The resistance at the circumference of the wheel is directly as the cube root of the fourth power of the weight of the load.

That is, representing the weights by } 1, 2, 3, 4, 5, 6, 7, 8, &c.;
the corresponding resistances will be } 1, 2.5, 4.3, 6.35, 8.55, 10.9, 13.4, 16, &c.

Whence it appears, that a weight of eight tons will produce sixteen times as much resistance at the circumference as a weight of one ton; and consequently, that there is a material advantage in carrying small loads, and increasing the number of carriages, instead of placing an immense load upon a single pair of wheels.

III. The resistance at the axis increases directly as the radius of the axis; or in other words, the resistance of an axis four inches diameter will be twice the resistance of one two inches diameter; and so on. Therefore, the smaller the axis can be made the better, so that it be of sufficient strength.

IV. When the direction of the moving power is parallel to the plane or road, the resistance at the axis is inversely as the radius of the wheel. That is, by doubling the radius of the wheel the resistance at the axis would be reduced one-half, and so on.

V. Also, when the direction of the moving power is parallel to the road, the resistance at the circumference of the wheel is inversely as the cube root of the square of the radius of the wheel. That is, representing a series of radii by the numbers

1, 2, 3, 4, 5, 6, 7, 8, &c.;
the corresponding resistances will be } $1, \frac{1}{1.55}, \frac{1}{2.05}, \frac{1}{2.52}, \frac{1}{2.9}, \frac{1}{3.3}, \frac{1}{3.66}, \frac{1}{4}, \text{ \&c.}$

From which it appears that the resistance decreases very rapidly as the height of the wheel increases. But in wheel-carriages drawn by horses the height of the wheels is limited. In a carriage moved by steam, the height of the wheels might be much increased; and it appears to me to be perfectly possible to construct such a machine that would move on the common roads.

VI. The resistance at the axis is inversely as the velocity. That is, a wheel moving with a velocity of three miles an hour will have double the friction at the axis that a wheel moving at the rate of six miles an hour will have.

VII. The resistance at the circumference of a wheel is as the cube root of the square of its velocity. Hence, if the velocities be expressed by the series 1, 2, 3, 4, 5, 6, 7, 8, &c.;

the resistance at the circumference } $1, \frac{1}{1.55}, \frac{1}{2.05}, \frac{1}{2.52}, \frac{1}{2.9}, \frac{1}{3.3}, \frac{1}{3.66}, \frac{1}{4}, \text{ \&c.}$
will be

Consequently, the resistance both at the axis and circumference decreases as the velocity increases. Common observation has satisfied

tified almost every one of the truth of this conclusion : yet there is one point connected with it which does not appear to be so well understood ; and which is,—that the same carriage will always do least injury to the roads when it moves with a considerable degree of velocity. For it has been shown that the depth of the impression is inversely as the square of the velocity (Prop. E). Consequently, the velocities being represented by the numbers 1, 2, 3, 4, 5, 6, &c.;

the corresponding de- } 1, $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{25}$, $\frac{1}{36}$, &c.
pression will be

But the injury done to the road must be as the depth to which the wheel sinks into it ; therefore the advantage gained in this respect by increasing the velocity is evident.

VIII. If the road were perfectly level, and of an uniform material ; the resistance at the circumference of the wheel would be inversely as the cube root of its breadth, while the resistance at the axis would not be altered. That is, representing the breadths by 1, 2, 3, 4, 5, 6, 7, 8, &c.;

the correspond- } 1, $\frac{1}{1.26}$, $\frac{1}{1.44}$, $\frac{1}{1.59}$, $\frac{1}{1.71}$, $\frac{1}{1.82}$, $\frac{1}{1.91}$, $\frac{1}{2}$, &c.
ing resistances
will be ..

Hence, on such a plane to increase the breadth eight times would reduce the friction one-half. But this will not apply to common wheel-carriages, because the roads are not uniform ; and it does not appear that any advantage can be gained by increasing the breadth of the wheel beyond a certain quantity, which depends on the size of the stones employed for making the road.

Such are the conclusions to which a theoretical view of the subject has led. It is easy to extend the same reasoning to inclined roads, and to cases where the line of traction is not parallel to the road ;—the resulting equations of course become more complicated, but they are not less important.

July 1, 1819.

THOMAS TREDGOLD.

IV. *Account of the Climate, Natural Products, Arts, and Manufactures of the Kingdom of Ashantee and some of the Territories adjacent.* By T. EDWARD BOWDICH, Esq.*

Climate.

DURING the first two months of our residence (May and June) it rained about one-third of the time ; throughout July and August it rained nearly half, and abrupt tornadoes were frequent in the evening just after sunset, ushered in by a strong wind from the south-west. The heaviest rains were from the latter end of September to the beginning of November ; they fell even in more

* Abstracted from Account of Mission from Cape Coast Castle to Ashantee in 1817. impetuous

impetuous torrents than are witnessed on the coast. The influence of the Harmattan was described as very powerful. Generally speaking, from the elevation of Ashantee (unfortunately we had no barometer), it was much cooler in Coomassie than at Cape Coast; indeed, from four to six in the morning there was a severity of cold unknown on the coast.

Natural Products.

The markets of Coomassie (the capital of Ashantee) were held daily from about eight o'clock in the morning until sunset. Amongst the articles for sale were beef (to us about eight-pence per pound), and mutton cut in small pieces for soup; wild hog, deer, monkey's flesh; fowls, and pelts of skins; yams, plantains, corn, sugar-cane, rice, *encruma* (a mucilaginous vegetable richer than asparagus, which it resembles), peppers, vegetable butter, oranges, papaws, pine-apples (not equal to those on the coast), bananas; large snails smoke-dried and stuck in rows on small sticks in the form of herring bone; eggs for fetish, pittò, rum, palm-wine, &c. &c.

A fruit called *boosie** is in great request. It is constantly chewed by the Ashantees on a journey; it is said to prevent hunger, and strengthen the stomach and bowels; has a slight bitter aromatic astringent taste, and causes an increase of the saliva while chewed. The boosie must be the gooroo-nut which Mr. Lucas describes as one of the articles of trade between Fezzan, Kassina, Bornoo, and the states south of the Niger. He writes: "Gooroo-nuts which are brought from the Negro states on the south of the Niger, and which are principally valued for the pleasant bitter that they communicate to any liquor in which they are infused:"—And again; "A species of nut which is much valued in the kingdoms to the north of the Niger, and which is called gooroo." It grows on a large and broad-leaved tree that bears a pod of about eighteen inches in length, in which are inclosed a number of nuts that varies from seven to nine. Their colour is a yellowish green; their size is that of a chesnut, which they also resemble in being covered by a husk of a similar thickness; and their taste, which is described as a pleasant bitter, is so grateful to those who are accustomed to its use, and so important as a corrective to the unpalatable or unwholesome waters of Fezzan and of the other kingdoms that border on the vast Zahara, as to be deemed of importance to the happiness of life. They are purchased at the rate of 12s. for 100 pods.

Sal-ammonia is found abundantly in Dagwumba. In the Ashantee market, a lump the size of a duck's egg was sold for 2s.; they grind it to mix with their snuff (of which they take large

* *Sterculia acuminata*, *Palis. de Beauvais, Flore d'Oware*, i. p. 41. tab. 21. quantities),

quantities), as it gives it a pungency agreeable to them. They also dissolve it in the water they give to their cattle, and sometimes drink it themselves for pains in the bowels. The Tamool practitioners in the East Indies suppose it to be a useful remedy in certain female obstructions and morbid uterine enlargements.

Mr. Lucas writes : " No commercial value appears to be annexed to the fleeces which the numerous flocks of the Negro kingdoms afford ; for the cotton manufacture which the Shereef says is established among the tribes to the south of the Niger, seems to be the only species of weaving that is known among them." In Dagwumba, however, they manufacture a coarse kind of blanket from sheep's wool.

There is a white grease which has long been called Ashantee grease by the natives on the coast, who supposed it to be produced in that country. They use it daily to anoint their skins, which otherwise become coarse and unhealthy. The Ashantees purchase it from the interior, and make a great profit by it ; it is a vegetable butter decocted from a tree called *timkëëa* ; it is doubtless the *shea* butter of Mr. Park.

The Ashantees procure most of their ivory from Kong, where they give eight ackies (or 40s.) in barter for a very large tooth.

The cattle we saw in Ashantee were as large as the English, unlike those on the coast, which resemble the Jersey. The sheep are hairy in Ashantee, but woolly in Dagwumba, an open country where they manufacture a close blanket. The horses in Dagwumba are generally small ; some were described to be fifteen hands high, but these were never parted with ; and the Ashantees did not desire them, for I never saw but one who rode fearlessly. The horses I saw were like half-bred galloways ; their legs lathy, with a wiry hair about the fetlock only requiring to be pulled. Their heads were large ; dun and mouse colours were said to be common ; they were never shod, and their hoofs consequently in the eye of the European, though not in the native, disproportionate ; they were fed on Guinea grass, occasionally mixed with salt, and sal-ammonia was frequently dissolved in the water. The saddles were Moorish, of red leather and cumbersome ; the bridles of twisted black leather thongs, and brass links with a whip at the end ; the bit severe, with a large ring hanging from the middle and slipped over the under jaw instead of a curb chain : the stirrups were like large blow pans and hung very short. Some of the Moors rode on bullocks with a ring through the nose.

The extent and order of the Ashantee plantations surprised us ; yet I do not think they were adequate to the population ; in a military government they were not likely to be so. They are chiefly

chiefly of corn, yams, ground-nuts, terraboys and eneruma; the yams and ground-nuts are planted with much regularity in triangular beds, with small drains around each and carefully cleared from weeds. They use no implement but the hoe. They have two crops of corn a year, plant their yams at Christmas, and dig them early in September. The latter plantations had much the appearance of a hop-garden well fenced in and regularly planted in lines, with a broad walk around, and a hut at each wicker gate where a slave and his family resided to protect the plantation.

All the fruits mentioned as sold in the market grew in spontaneous abundance, as did the sugar-cane; the oranges were of a large size and exquisite flavour. I believe this fruit has hitherto been considered as indigenous to India only. We saw no cocoa-nut trees, nor was that fruit in the market. Mr. Park's route was through a very different country. In the marshy ground a large species of fern is very abundant; there are four varieties of it: in shady places that have been cultivated, various tribes of *urtica*; and the *leontodon* grows abundant to the north of Coomassie. The miraculous berry which gives acids the flavour of sweets, making limes taste like honey, is common*. The castor oil (*Ricinus communis*) rises to a large tree; I have only seen it as a bush about three feet high on the coast; and the wild fig is abundant, though neither of them is used by the natives. The cotton plant is very plentiful, but little cultivated. The only use to which they apply the silk cotton is to the stuffing of cushions or pillows†. Mr. Park observed the tobacco-plant which grows luxuriantly in Inta and Dagwumba, and is called *poah*. The visitors from those countries recognised it in a botanical work. They first dry the leaves in the sun; then, having rubbed them well between their hands, mix them with water into oval masses. The Ashantees, however, never use this tobacco when Portuguese tobacco can be got from the coast even at the most extravagant rate. They will sometimes give two ounces of gold for the roll of Portuguese tobacco. The Dutch governor-general has been known to receive eighty ounces of gold from the Ashantees for tobacco alone.

Lions are numerous on the northern frontiers of Inta; elephants are remarkably numerous in Kong, and they are also found in

* The curious fruit to which I have given the name of *oxyglycus*, I find, was known to De Marchais, who describes it as a little red fruit, which being chewed gives a sweet taste to the most sour and bitter things.—Dalzel's *Dahomey*.

† Cotton of the cotton tree (or silk cotton) *Bombax pentandrium*, Linn. This cotton is not used for thread, but is made into pillows and beds. It is also, from its catching fire so easily, commonly put into tinder-boxes and employed in the preparation of fire-works.—Ainslie's *Materia Medica of Hindostan*.

Ashantee, with wild hogs, hyænas, cows, sheep, goats, deer, antelopes; dogs approximating to the Danish; cats extremely sharp-visaged and long-necked; Gennet cats, pangolins, alligators. The rhinoceros is found in Boroom, and the hippopotamus in the Odirree river.

The Ashantees say that an animal called *sissah* or *sissiree* will attack every other however superior in size. The Fantees, who had never seen it, had imbibed a tremendous idea of it from the stories in their own country. I doubt its being so formidable to all other animals, for the skin I saw was not more than three feet long and the legs short; it resembled that of a boar, but the natives said it was between a pig and a goat. It is extraordinary that the *gnoo* (antelope *gnu*) which is found behind the Cape of Good Hope is known in Inta by the same name. Where the beds were not an accumulation of cushions, the skin of the *gnoo* was nailed to a large wooden frame, raised on legs about a foot from the ground, and stretched as we would sacking. It was a revered custom that no virgin of either sex should sleep on this kind of bed. Another animal called *otrum* was described by the inhabitants of the eastern frontier as having one very long horn on one side of the head, and a short one on the other; it is much larger than the *gnoo*. We met with a spotted animal of the cat kind, very common, and allied to the leopard or panther; but whether referable to either of those species, or to be considered as distinct, we could not determine, owing to the very vague and unsatisfactory character by which naturalists have attempted to distinguish them,—the kind and numbers of the rows of spots; which we have observed in individuals of the same decided species to present almost an infinity of variation.

The vulture, which is venerated by the natives for the same reason that the Egyptians venerated the *Vultur percopterus*, is the *Vultur monachus* figured by Le Vaillant. Green pigeons are found, and crows with a white ring round their necks, probably the *Corvus scapularis* figured by Le Vaillant. There were several small birds of beautiful plumage which sung melodiously; two in particular, the one like a blackbird, and the other of the same colour as the English thrush, but larger. Also a variety of parrots beautifully spangled with different colours. M. Cuvier was misinformed when he wrote (*Regne Animal*, tome i. p. 108) “Macaque est le nom generique des singes à la côte de Guinée.” The name is unknown there as well as in the interior. *Dokoo* is the generic name. The *Símia Diana*, which has the most beautiful skin of any monkey, is found in Ashantee as well as in Warsaw. All the natives agree that they do not know of any monkeys which dare to attack men but the *akoneson*, which they describe as small and always seen in troops.

Snakes

Snakes green and of all colours ; scorpions, lizards, &c. were found as on the coast, with a curious variety of beetles and the most beautiful butterflies. A few specimens preserved in spirits have been sent to the British Museum.

Arts and Manufactures.

The Ashantee loom is precisely on the same principle as the English ; it is worked by strings held between the toes ; the web is never more than four inches broad. A small loom complete is among the articles presented to the British Museum. They use a spindle and not a distaff for spinning, holding it in one hand, and twisting the thread (which has a weight at the end) with the finger and thumb of the other. The fineness, variety, brilliancy and size of their cloths is astonishing ; a specimen which is in the British Museum will be admired for the two first qualities, and for having the same appearance on both sides. The richest red taffetas imported from India are unravelled, and wove into the cloths of their own manufacture. They are also sometimes in the custom of unravelling a few of the fancy silks (India), but these are generally bought for wear, though they prefer those from Fezzan for that purpose, because the colours are more showy. The richest silks I saw were worn by the Moors, who had bought them at Yahndi and Houssa. Reckoning nine inches to a span, there are eight spans in a fathom, which is the Ashantee measure ; but the fathom of Inta and Dagwumba contains only six spans. Even if the Ashantee traders give only twenty shillings a fathom in barter of boosie, salt, rum, iron, &c. it is considerably cheaper than ours (silks), considering that they get 100 per cent. on it at Coomassie. Mr. Lucas mentions " silk wrought and unwrought " among the articles exported from Fezzan to Kassina. Apokoo and several others related to me that Sai Cudjo bought a piece of silk at Yahndi so very fine, that although it could be compressed between two hands, it was nevertheless larger than any cloth I had seen the present king wear, and his appeared monstrous. Apokoo added that six slaves were paid for it, which would have produced 160*l.* at the water side.

A description of British cotton cloth (which goes here by the name of *sarstracunda*) is in considerable request. It is a highly glazed article, of bright red stripes with a bar of white, and is bought solely for the red stripe (as there is no red dye nearer than Marrowa), which the Ashantees weave into their own cloths, throwing away the white. The red dye of Marrowa, which is very good, is obtained from a tree called *mogsaralee*.

The white cloths which are principally manufactured in Inta and Dagwumba they paint for mourning, with a mixture of blood
and

and a red dye wood. The patterns are various and not inelegant, and painted with so much regularity with a fowl's feather that they have all the appearance of a coarse print at a distance. I have seen a man paint as fast as I could write. There is a very fair specimen in the British Museum, the price of painting which was one ackie.

They have two dye woods, a red and a yellow, specimens of which I brought down. They make a green by mixing the latter with their blue dye, in which they excel; it is made from a plant called *acassie*, certainly not the indigo which grows plentifully on the coast. The *acassie* rises to the height of about two feet, and, according to the natives, bears a red flower; but the leaf is not small, fleshy or soft, nor is it pale or silvery coloured underneath; it is a thin acuminate leaf about five inches long and three broad, of a dark green. The shrub has opposite leaves, no stipules, and has a certain degree of resemblance to *Marsdenia suaveolens* (the indigo of Sumatra); but as the leaves are toothed in the *acassie*, it probably does not belong even to the same natural order. I regret to add, that our best specimens of this plant perished in the disasters of our march, and no drawing was made of it, as it bore no flower in that season; it grows abundantly in the woods, and produces a fast and beautiful colour without requiring a mordant. They gather a quantity of the leaves, bruise them in a wooden mortar, and spread them out on a mat to dry: this mass is kept for use; a proportion of it is put into a pot of water, and remains six days previous to immersing the thread, which is left in six days, drying it once every day in the sun; it is then a deep lasting blue colour. When a light blue is wished for, the thread is only allowed to remain in the pot three days.

They excel in pottery, as the pipes in the Museum will show; they are rested on the ground when smoked; the clay is very fine, polished (after baking) by friction, and the grooves of the patterns filled up with chalk. They have also a black pottery which admits of a high polish.

The people of Dagwumba surpass the Ashantees in goldsmiths' work, though the latter may be esteemed proficient in the art. The small articles for the Museum—a gold stool, sanko bell, jaw-bone and drum—are not such neat specimens as I could wish; the man who made them having too much costly work on hand for the king, to pay our trifles his wonted attention; unfortunately too he was committed to prison before they were quite finished; however, they will give an idea. I weighed out nineteen ackies and a half of gold-dust for making these articles; one-third of an ackie was lost in melting, and five was the charge of the goldsmith. Bees'-wax for making the model of the article wanted is spread out on a smooth block of wood by the side of a fire, on which

which stands a pot of water; a flat stick is dipped into this, with which the wax is made of a proper softness; it takes about a quarter of an hour to make enough for a ring. When the model is finished, it is inclosed in a composition of wet clay and charcoal (which being closely pressed around it forms a mould), dried in the sun, and having a small cup of the same materials attached to it, (to contain the gold for fusion,) communicating with the model by a small perforation. When the whole model is finished, and the gold carefully inclosed in the cup, it is put in a charcoal fire with the cup undermost. When the gold is supposed to be fused, the cup is turned uppermost, that it may run into the place of the melted wax; when cool the clay is broken, and if the article is not perfect it goes through the whole process again. To give the gold its proper colour, they put a layer of finely-ground red ochre (which they call *juchuma*) all over it, and immerse it in boiling water mixed with the same substance and a little salt; after it has boiled half an hour, it is taken out and thoroughly cleansed from any clay that may adhere to it. Their bellows are imitations of ours; but the sheep skin they use being tied to the wood with leather thongs, the wind escapes through the crevices; therefore when much gold is on the fire they are obliged to use two or three pair at the same time. Their anvils are generally a large stone, or a piece of iron placed on the ground. Their stoves are built of sursh (about three or four feet high) in a circular form, and are open about one-fifth of the circumference; a hole is made through the closed part, level with the ground, for the nozzle of the bellows. Their weights are very neat brass casts of almost every animal, fruit, or vegetable, known in the country. The king's scales, blow-pan, boxes and weights, and even the tongs which hold the cinder to light his pipe, were neatly made of the purest gold that could be manufactured.

Their blacksmiths' work is performed with the same sort of forge as the above; but they have no idea of making iron from ore, as their interior neighbours do. Their swords are generally perforated in patterns like fish trowels; frequently they make two blades springing parallel from one handle which evince very fine workmanship. The needles and castanets will only give some idea of their progress. The iron-stone is of a dark-red colour, spotted with gray and intermixed with what has all the appearance of lava; they cut bullets out of it for the army when lead is scarce. I have brought some arrows of native iron. They have no idea of making a lock like the people of Houssa and Marrowa.

They tan or dress leather in Ashantee, but they do this and dye it in a very superior manner in Houssa and Dagwumba;—see the sandals and cushion in the British Museum, the former varied

and apparently stitched: doubting that there could be such stitching, I undid a part, and discovered that they perforated the surface, and then stuck in the fine shreds of leather. The curious will observe that the patterns of the stool cushion are all produced by paring the surface. They make their soldiers' belts and pouches out of elephant or pig skin ornamented with red shells.

Of their carpenters' work the stool is a fair specimen, being carved out of a solid piece of a wood called *zesso*, white, soft, and bearing a high polish; it is first soaked in water. They sell such a stool for about three shillings; in Accra or Fantee it would fetch twenty. The umbrella is even more curious; the bird is cut almost equal to turning, and the whole is so supple that it may be turned inside out. This (only a child's umbrella) is a model of the large canopies I have described in the procession; I gave a piece of cloth value twenty shillings for it. The sanko or guitar is also neatly made, and the chasteness and Etruscan character of the carving is very surprising. The surface of the wood is first charred in the fire, and then carved deep enough to disclose the original white in the stripes or lines of the patterns.

Numbers of workmen are employed in breaking, rounding and boring the snail shells, as big as a turkey's egg generally, and sometimes as large as a conch. They are first broken into numerous pieces; then chipped round, the size of a sleeve-button, and afterwards bored with a bow and iron style fixed in a piece of wood. Lastly, they are strung and extended in rows on a log of wood, and rubbed with a soft and blueish gray stone and water, until they become perfectly round.

Their pine-apple thread is very strong, and is made from the fineness of a hair to the thickness of whip-cord; it bleaches to a beautiful whiteness, and would answer for sewing any strong materials; but when muslin is stitched with it, it is liable to be cut from the harshness. The women frequently join their cloths and ornament their handkerchiefs with a zigzag pattern worked with unravelled silks of different colours. The fetish case is a specimen of their needle-work in the manner of chain stitch.

V. *On finding the Longitude by Lunar Observations.* By
Mr. HENRY MEIKLE.

To Mr. Tilloch.

London, June 15, 1819.

SIR, — **T**HE problem of finding the longitude by the lunar observations has long and justly been regarded as an interesting subject. To accomplish this desirable end, various methods of reducing the lunar distances have been proposed with different degrees of success. But while methods very different agree in
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bringing out the same result, it may also be remarked, that many methods apparently different are essentially the same; the seeming difference arising only from the endless variety of forms under which the quantities may be analytically expressed. Hence many have fancied themselves the inventors of new methods, who are little, if in any degree, entitled to the name.

The pretensions to accuracy are likewise of very different degrees: some of these being also of the imaginary kind; for some of the greatest efforts to attain exactness are frequently productive of greater errors than those they were intended to remove. Thus, where perfection is aimed at, an allowance is made on the sun and moon's altitudes for the effect of refraction in diminishing their vertical semidiameters, without considering that the centre is seldom the apparent place of the angular point of the spherical triangle;—sometimes also an allowance is made for the contraction of that semidiameter which is to be applied to the distance. This, as we shall afterwards see, is likewise an elaborate way of creating new errors.

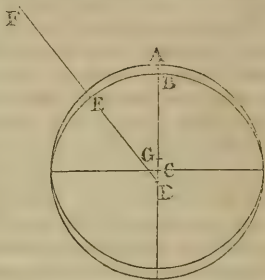
But at any rate, however great may be the distortion of the disk by refraction, this does not in the least affect the true altitude of the centre, when the other corrections are properly applied; for, if to the observed altitude of the limb, we first apply the refraction, &c. we get the true altitude of the limb; and if to this, the horizontal semidiameter be afterwards applied, it gives the true altitude of the centre absolutely free from such an error. This, though hinted at by different authors, seems to be little, if at all, adopted in practice; for in books of navigation it is customary first to apply the semidiameter to the observed altitude of the limb, as if that would accurately give the apparent altitude of the centre, and then with this altitude to take out the corrections.

Some sort of respect, it is true, may in this way be paid to the centre of the luminary; but it is only a needless source of error, which might be avoided without any additional labour. It would hence appear, that the sun and moon's "true altitudes" when about 7° are, in addition to other inaccuracies, subjected to an unnecessary error of $18''$ by the usual careless way of working. But, as we shall afterwards see, this is only half the extent to which the error may go with the apparent altitude. It is in vain that we expect accuracy even from the best observers and instruments, if such needless errors are persevered in.

Although the common way of first applying the semidiameter does not accurately give the apparent altitude of the centre; yet in some cases it gives that of a point which is very near to that point of the vertical diameter through which the continuation of the arc representing the apparent distance would pass, especially

when sensibly affected by the distortion of the disk. Now the position of this point is evidently of more importance than that of the centre itself; because the centre is not necessarily the apparent place of the angular point of the triangle.

Suppose the vertical semidiameter of the moon to be shortened by the small space AB. Then if we take the distance of the moon's limb at E, from an object which lies in the direction EF, this arc EF will be perpendicular to the limb at E, and if produced, will cut the vertical diameter at some point D, which is below the centre C, by a space nearly equal AB. For the upper part of the disk will not sensibly differ from a circle whose radius is equal the



semidiameter parallel to the horizon, unless the altitude be very small. If, therefore, we have observed the altitude of the upper limb, the place of D (not the centre) is found by subtracting the augmented semidiameter. But when the distance has been taken from a part of the disk on a different side of the diameter which is parallel to the horizon from the limb whose altitude has been observed, then this method fails; for in the present case, had the altitude of the lower limb been observed, and the semidiameter added, it would have given the altitude of a point G as much above the centre as it ought to be below. When this takes place, recourse must be had to a table answering to the space DG. From this it is evident, that when the apparent altitude of an angular point of the spherical triangle is about 7° , it may sometimes, in the common way of working, be erroneous by $36''$; and even in the method hitherto used for remedying this evil, the error will rarely be less than $18''$ at the altitude of 7° . But both are much more considerable when the altitude is smaller.

The method referred to, for correcting the altitudes for the distortion of the disk, by a table answering to the diminution of the vertical semidiameter, is thus productive of an evil little inferior to the one it was intended to remove; for although by it the true altitude of the centre were obtained, and also its apparent altitude; yet, as has been remarked, the centre is seldom the point from which the apparent distance should be reckoned; so that what is gained in getting the true altitude of the centre is just lost in departing from the point from which the distance should be taken; and to which, as we have already seen, the common way of working often makes a much nearer approach.

With regard to the line DE, which some learned authors have
been

been pleased to consider as shortened by refraction, the mistake has no doubt arisen from the old established habit of always estimating the apparent distance from the centre. But if we suppose the disk to be elliptical*, then it may be shown, that if from any point of either axis of an ellipsis but the centre, a straight line be drawn perpendicular to the curve,—this line, sometimes called the *normal*, is greater or less than half the other axis, according as the point was taken in the conjugate or transverse axis. DE is, therefore, always greater than the greatest semidiameter; so that it is obviously much safer just to account it equal the augmented semidiameter than less.

I do not mean to insinuate that I have put this subject beyond the possibility of improvement, or that there may not be defects in the above way of considering it. My object is merely to make a nearer approach to accuracy with as little additional labour as possible. The method which I would therefore recommend when the distance and altitude have both been measured from parts of the limb which are either both above or both below the centre—is, first, to find the true altitude of the limb, to which the true or horizontal semidiameter being afterwards applied gives the true altitude of the centre. Next to the observed altitude of the limb apply the “augmented semidiameter,” which will give the place of D; and then with these compute the true distance as usual. But when the distance and altitude have been observed from different sides of the diameter parallel to the horizon, it will be necessary that the correction contained in several books for the contraction of the *whole* vertical diameter be subtracted from the semidiameter which is to be applied to the apparent altitude of the limb in order to get the place of D.

It may be proper to anticipate an objection which might be brought against this method—that when the angle at the sun or moon is a right angle, the centre is the angular point of the triangle; and the above rule would then make the altitude erroneous by half the contraction. This cannot be denied: but it is no less obvious, that a small alteration on the altitude at a right angle will have no sensible effect on the distance, which is the main thing to be attended to. If greater accuracy were required, a careful determination of the figure of the disk would probably point out the construction of a table answering more correctly to DG, and which might be applicable to different angles formed at the luminaries. A table could likewise be formed which would give the excess of DE above the semidiameter for different angles

* The figure of the sun or moon when near the horizon will more nearly consist of two semi-ellipses having the transverse axis common to both; but the semi-conjugate axis of the upper half greater than that of the lower. It is easy to see, that the above theorem applies on this supposition also.

and altitudes. But this is a degree of nicety too laborious for common practice.

It will probably be alleged that much has been said about a small correction,—that of removing the effects of the contraction of the sun and moon's disk. But if this degree of accuracy may be attained with little or no additional labour, I think it ought not to be neglected; and the more especially as there are probably abundance of remaining inaccuracies not so easily dislodged from the lunar distances. Besides, I do not consider an error of seven or eight minutes of longitude unworthy of avoiding, when it can be done so easily; and a much greater error may sometimes be produced by neglecting the quantity under consideration. Toward the equator, where the errors depending on the figure of the earth vanish, this one may still have its maximum effect on the lunar distance.

Among other vague things so abundant in books of navigation, it is customary to say—that “the limb of the Hadley's quadrant contains only 45° or the eighth part of the circle; but that by reason of the *double* reflexion the angle is *doubled*.” It would, indeed, be difficult to give a more insignificant description of this instrument; for it is easy to show, that the *doubling* (or rather *halving*) is completely effected by the first reflexion: the second serving no other purpose than that of giving the rays a more convenient direction. It might perhaps have at least some meaning, to say—“that the limb contains only 45° , for by reason of the reflexion of the index-mirror, the instrument only gives half the observed angle.” But our authors on navigation are generally so well trained to follow in the old-beaten track, that they run little risk of wandering into any improvement.

It is a common popular doctrine, that parallax operates only in a vertical direction. But if this were true, it would make the moon appear of an oval figure, which, as is easily shown, cannot be the case; for it is obvious, that if the moon be spherical she must appear circular, let the observer go where he will, except so far as depends on the intervening medium. The difference of the parallaxes for any two diametrically opposite limbs, constitutes what is called the *augmentation* of the diameter.

Another thing which claims strong reprobation, is, the method of finding the latitude by a table of “difference of altitude of polestar and pole.” This method is always erroneous, unless the latitude be near to 0; or, when the polestar is in the meridian—a case where the aid of such a helpless table is not wanted.

In this method it is liberally supposed, that the altitude of the polestar when six hours distant from the meridian is the same as that of the pole. This, however, is no where true but at the equator; and the reason is obvious: for in any other case when the
star

star is six hours from the meridian, its zenith distance, which is less than 90° , being the hypotenuse of a right-angled spherical triangle, is therefore greater than the zenith distance of the pole, which is only a side of that triangle.

But another grand fault is—that it makes no provision for the rapid change of polar distance to which this star is subject. Had it no other fault, this alone would render such a table vague enough in the course of a very few years. In some very respectable and scientific works a table of this sort is given, in which the polar distance of the polestar (involved in the whole table) is erroneous by $4'6''$ at the time of publication; and as the error increases nearly one minute in three years, it is not difficult to perceive what confidence ought to be put in such tables.

All the errors and delusions I have noticed are more or less indebted to the *original* system of *copying* the same things over and over again, without ever inquiring whether they have a good foundation, or indeed any foundation at all. Such is the contented and unsuspecting disposition of modern authors, that they can safely take each other's word for things which in less enlightened times would have required a demonstration. Suspicion, the very bane of society, becomes an exalted virtue when applied to science. I am, sir,

Your most obedient servant,

HENRY MEIKLE.

VI. On *Aërolites*.

To Mr. Tilloch.

London, June 17, 1819.

SIR, — THE very curious question of *aërolites* has lately been introduced into your pages by MM. Capel Lofft and Acton; and though I seldom indulge in speculative opinions on scientific subjects, being more desirous of confining myself to a simple detail of experiments and facts, I am induced to digress a little from that line in the present instance.

You may remember that in one of my former papers I adverted to a meteoric stone which fell at Pulrose in the Isle of Man. The evidence which I collected seemed to attest the fact, and place it beyond a doubt; and yet its physical characters of extreme levity and scoriaceous texture, seemed to impose a doubt upon its identity with those stony masses which have at different times visited the earth; and this comparison, coupled with an examination of those *aërolites* which I had the opportunity of seeing in many museums on the continent, did not tend to unhinge that doubt.

In this uncertainty of mind I turned over the leaves of the

Journal de Physique, and found in volume lx. a description of some spongy stones which fell near Roa, not far from Burgos in Spain, in 1438. The analogy is very striking, and I do not know any other instance upon record which describes meteorolites under a similar character. It may be considered as corroborating that which I have detailed, and confirming its claim to a similar origin:—“Mais ce qui causoit le plus d'étonnement, c'étoit leur excessive légèreté, puisque les plus grandes ne pesoient pas une demi-livre. Elles étoient si tendres, qu'elles ressembloient plus à de l'écume de mer condensée qu'à toute autre chose. On pouvoit s'en frapper le dedans des mains sans crainte d'y causer ni contusion ni douleur ni la moindre apparence, &c.” Now this is precisely the case with that in question.

I confess that I read with some degree of astonishment Mr. Brande's opinion on the origin of meteoric stones (*Journal of Science and the Arts*, No. 10, page 294), because I believed that their supposed lunar origin had been generally abandoned, and that the opinion which confined them to our atmosphere had ceased to be problematical. Mr. Brande observes, after stating the questions of their being “earthy matter fused by lightning?” or “the offspring of a terrestrial volcano?” and adverting to the inexplicable projectile force that would here be wanted, that “this is merely explaining what is puzzling by assuming what is impossible,” and that, in this conjecture, the advocates of such an opinion “have assumed one impossibility to account for what they conceive to be another.” The Professor then continues: “The notion that these bodies come from the moon, though it has been laughed at as lunacy, is, when impartially considered, neither absurd nor impossible. — It is quite true,” says he, “that the quiet way in which they visit us is against such an origin; it seems, however, that any power which would move a body 6000 *feet in a second*, that is, about three times the velocity of a cannon-ball, would throw it from the sphere of the moon's attraction into that of our earth. The cause of this projective force may be a *volcano*; and if thus impelled, the body would reach us in about two days, and enter our atmosphere with a velocity of about 25,000 *feet in a second*.” With every respect for Professor Brande's opinion, and highly as I may value it, it is impossible for me to receive the present in any other than a very doubtful form.—Mr. B. denies to any volcano on the terrestrial surface a projectile power of extent sufficient to propel volcanic dust to the required elevation; and yet unhesitatingly gives to lunar volcanoes one not merely of enormous, but of almost inconceivable impetus. Now, to say nothing of the “quiet way in which they visit us,” the very existence of lunar volcanoes, to state the least of it, is exceedingly questionable. Sir W. Herschel

schel thought he saw the gleam of a volcano on the lunar sphere; and this being of the imagination has been received as currency, and made the pedestal of the hypothesis which regards these "children of the air" as of lunar formation.—Now here, methinks, two difficulties are introduced; namely, That the lunar mass is composed of very different materials from any we find on our own planet,—and a necessity of admitting the existence of volcanoes in the moon gifted with powers of projection of the most extraordinary description. Now, could the bright speck which our eminent astronomer witnessed, proceed from no other source than a lunar volcano?—The celestial exhibitions which from time to time greet the eye, should tend to mingle humility in our estimate of the phænomena of the stars. Besides, many eminent astronomers have denied the assertion of Sir W. Herschel, and among them we find the name of M. Arago. The existence of a lunar atmosphere seems necessary for this position; and the occultations of the stars clearly prove that such an atmosphere does not exist, or at least that it does not possess a sensible density. Moreover, on this supposition meteorolites ought to be confined to a given range; whereas, so far from being limited to any parallel of latitude, they are not bounded by any prescribed lines. It seems less difficult to say what they are not, than what they really are: but I think a proper consideration of the phænomena which announce their presence and their fall, might in some measure serve as a clue to guide us out of the difficulty. Their precipitation from the atmosphere, though once deemed fabulous, is no longer doubted, and the identity of their elements and peculiar aggregations pronounces their common origin, and that they are not terrestrial bodies. A black cloud, the splendours of the thunder-storm, the apparition of light succeeded by tremendous explosions, are the heralds of their fall; and when it occurs at night, a luminous phænomenon proclaims their passage through the regions of the atmosphere. Mr. Brande thinks that "their ignition may be accounted for either by supposing the heat generated by their motion in our atmosphere sufficient to ignite them; or, by considering them as combustibles ignited by the mere contact of air." It is very true, we may assume any thing; and the rapidity of their progress, on the supposition that they are launched from the surface of the moon and enter our atmosphere with the velocity of "about 25,000 feet in a second," might, by the extraordinary friction that would obtain, be sufficient to ignite them;—but to be inflamed by the mere contact of cool air in passing through that rarefied medium, where even aqueous vapour may lose its elasticity and become solid matter, we must *suppose* that the meteoric stones are entirely composed of some such body as *potassium*:—now they are well known ag-

gregations

gregations of iron, nickel and chromium, with silica, alumina and magnesia; and occasionally sulphur, lime and carbon, more rarely containing traces of cobalt. As for the possibility of the *creation of iron*, &c. from simpler forms of matter, which Mr. Brande also presumes, though supposititiously, I need only again repeat that we are at liberty to suppose what we will. In presuming my opinion in opposition to that of Professor Brande I shall not be so extravagant, but deduce what I may consider fair data from the history of meteorolites, and the circumstances which announce and accompany their fall.

I think it evident that these meteoric bodies are not wandering masses circulating round our globe, nor chips from any planet moving within the compass of the solar system. But I will not say either that they are the gifts of our atmosphere, or extra-atmospheric, and formed on the surface of its outer shell.

It is surely more reasonable to suppose that they spring from the thunder-storm, than that the storm is the result of their action.

They do not penetrate far into the earth; sometimes are simply scattered on its surface. Now, if the elevation at which they were formed was very considerable; on the well-known laws of gravitation, they should sink deep into the soil.

Though the peculiar *proportional* associations are not discoverable in any terrestrial mineral, still the substances themselves are not rare in the earth.

As to the agency which carried them to such lofty elevations, I cannot find it either in volcanic forces or the whirlwind from the desert; nor do I discover in volcanic products materials for their construction.

Hydrogen variously combined is continually escaping from all parts of the surface of the globe; sometimes it carries on its wing iron or carbon or sulphur or other materials; and who shall decide that nickel in intimate chemical combination with iron, or silica with oxide of iron, &c. may not be transported in such a vehicle? And the combined hydrogen might in virtue of its great levity, and expanding as it ascended, finally brave the outer circle of the atmosphere and settle upon its waves.—We have no right to limit its solvent powers, and it is still questionable whether it has ever yet appeared to the chemist in a simple form. We know it dissolves iron, zinc, arsenic, tellurium, selenium, potassium, boron, carbon, sulphur, and phosphorus; and we have no authority to restrict its powers to even these. As for oxygen*, &c.—for any thing I know they may have very exalted solvent powers.

We have then only to suppose two immense aerial volumes

* Professor Giabert of Turin told me he had found invariably, that oxygen obtained from peroxide of mercury, when respired, produced *salivation*.

loaded with such materials as these, and floating either in or on the atmosphere: they would be *differently* electrified; for oxygen with its contained materials, and hydrogen with its accompaniments, would certainly be so.

The effect of two such masses coming into collision, would be exhibition of light and communication of heat.—The two electricities rushing into contact would produce explosion; the gases would be ignited*; the stony materials undergo fusion,—and in that moment the formed *aërolite* would take its flight to the earth.

As to the change in the proportional constituents, the powers of electricity will account for this. Has not Sir H. Davy proved that the usual powers of nature may be controlled by its action? By giving foreign electric properties to alkalies and acids, has he not changed their relations, and destroyed their natural attractions?

The metals concerned in this extraordinary fabric are susceptible of receiving magnetic phenomena. Electricity permanently identified with iron or nickel may impart to them this property, vested in only a privileged few. A fine steel bar electrified becomes magnetic, and the action of lightning has reversed the polarity of the mariner's compass.

Dr. Clarke has found that meteoric stones become magnetic after fusion by the oxyhydrogen blow-pipe, which may proceed from a volatilization of the sulphur; for it is known that the presence of either arsenic or sulphur weakens or destroys this susceptibility. I have the honour to be, sir,

Your very humble and most obedient servant,

J. MURRAY.

VII. *Observations on the Study of Mineralogy.* By ROBERT BAKEWELL, Esq.†

MINERALOGY is a branch of natural history; and he who would obtain a just and comprehensive view of the subject, must not confine his attention to minerals as they are arranged in cabinets, but contemplate them as they occur in their native repositories; he should endeavour to trace the connexion between different species of minerals, and the changes which they undergo by processes of natural chemistry—changes which cannot at present be imitated in our laboratories. Persons who live principally in

* The oxidation of the thin shell of the *aërolite* is in harmony with this view. Aqueous vapour would be formed by the ignition of the gases, and impart that appearance which it has received from some such means.

† Extracted from the Preface to Mr. Bakewell's Introduction to Mineralogy just published.

large cities, and only view minerals in cabinets, are led to entertain the idea, that in the mineral kingdom, nature is in a state of profound repose, and that all the different minerals at present existing, are coeval with the globe itself. It is true, that when minerals are taken from the mine and placed in cabinets, they appear to undergo no further change, and to be imperishable; but in their native repositories, changes are constantly though slowly taking place; they increase in size, advance to maturity, and afterwards decay more or less rapidly, though the life of a mineral, if we may use the expression, extends far beyond the duration of life in animals or vegetables.

It is worthy of particular attention, that certain species of minerals, very dissimilar in their composition, are almost always associated together. Now, to use the words of the late Bishop of Landaff, "Though it may be too much to infer, that one of these substances arises from the natural decomposition of the other, juxtaposition in the bowels of the earth being no certain proof of their being derived from each other; yet the mind cannot help conjecturing, that a more improved state of mineralogy will show some connexion in their origin."—*Chemical Essays*, vol. iv.

When the same species of mineral from different parts of the world always contains a certain substance that appears to the superficial observer to be foreign to it; the common origin of both substances may be inferred with still greater probability. Thus galena (the common ore of lead) almost invariably contains a portion of silver. This is the case with the common lead-ore in every part of England, though not a particle of silver-ore has been discovered there, except in Cornwall and Devonshire. Such facts particularly deserve the attention of the philosophical mineralogist, and I have been careful to notice them, in order to excite a spirit of inquiry. Can we say in the above instance, that the lead was forming into silver, but was arrested in its progress; or is the change now taking place? I do not deem it unphilosophical to believe, that the vivifying influence of creative energy descends to the deepest recesses of the earth, acting according to laws as regular as those which govern the motion of the planets in the heavens.

"*Mens agitat molem, et magno se corpore miscet.*"—VIRG.

————— 'One pervading soul
Fills the great mass, and mingles with the whole."

The combined effects of magnetism, electricity, crystalline polarity, and chemical affinity, are probably united with other causes at present unknown, producing all the various changes in the mineral kingdom, including volcanic phenomena. But in whatever manner these changes are effected, they are undoubtedly going

going on; and he who views the mineral kingdom as an inert mass of heterogeneous elements, and confines his attention exclusively to cabinet collections, in order to obtain a knowledge of nature, is "seeking the living among the dead."

Unfortunately the objects of mineralogical research are placed at a considerable distance from the residence of men of science, who are therefore seldom able to observe them in their native repositories, and working miners are little qualified to describe the phenomena presented to their notice.

It was said by one of the ancients, that the world would be well governed whenever kings were philosophers, or philosophers became kings. We may say with greater truth, that mineralogy will become a perfect science, whenever working miners are enlightened mineralogists, or enlightened mineralogists become working miners: but as these events are beyond the sphere of probable occurrences, neither the politician nor the naturalist need stop to anticipate the result of their accomplishment.

The attention of mineralogists has been too much devoted to the discovery of new species that possess no importance in nature, and can be of no use in the arts; or they have been engaged in the useless labour of inventing new names, and classing as new species every variety they meet with, attaching the names of distinguished characters, to minerals which have neither use nor beauty to recommend them to our notice. Can Werner or Haüy derive honour from having their names affixed to such minerals? What should we think of the taste or good sense of the naturalist who affixed the names of Linnæus, Lamarck, or Cuvier, to any newly-discovered variety of gnat, flea, or bug? But a similar absurdity is frequently committed by mineralogists. This frivolous practice of changing and multiplying names probably originated with mineral dealers on the continent, who were thus enabled to multiply their specimens, and to obtain a high price for substances which possessed no recommendation whatever, but their supposed rarity.

As some apology for the attention which has been devoted to substances of little use or importance on account of their scarcity, it may be right to remark, that in the present state of the science, mineralogists are like the searchers for diamonds in the mines of India, who are obliged to pass every stone between the eye and the light, lest they should inadvertently throw away a diamond. That I may part in good temper with those who attach more importance than I do to the discovery of new mineral species merely on account of their rarity, I shall beg leave to conclude with a few observations connected with the subject that I have sometimes delivered in my lectures, when treating on crystalline and magnetic polarity:—We ought to be extremely cautious in de-
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nying the value of any discoveries in nature or in science, because their application to useful purposes may not be immediately obvious. How greatly would the heroes and statesmen of antiquity have despised the labours of that man who devoted his life to investigate the properties of the magnet ! Little could they anticipate that this humble mineral was destined to change the very form and condition of human society in every quarter of the globe. Let us bear in mind, that the magnet was known nearly two thousand years before it was applied to any purpose of practical utility. During that time the ocean was a fearful and trackless abyss, and voyages were confined to short excursions along the shore. The wanderings of Ulysses, which took ten years to accomplish, did not exceed in extent and difficulty the voyage from Dublin to London. When magnetic polarity was applied to the aid of navigation, mankind seemed to have acquired a new sense ; guided by this mysterious power, Vasco de Gama and Columbus led the way to the eastern and the western world, and a fresh impulse was given to the human mind, which had been slumbering for centuries. There is not a country inhabited by man where the influence of this discovery has not been felt ; it has been the parent of our naval and commercial greatness, and has obtained for us the commanding rank we hold among the nations of Europe. The history of the magnet should teach us not to despise any discovery in science because its utility cannot be immediately perceived. Some discoveries are unproductive, until the progress of science in a future age directs their application to purposes of eminent utility. Such discoveries may be compared to the acorn, which may remain for a time buried in an arid soil, until, stimulated by the genial rains of heaven, it strikes its roots into the earth, and springs forth to day—humble indeed in its first appearance, and slow in its progress to maturity ; but destined at length to become a mighty oak, stretching its arms amid the skies ; the ornament, the strength, and the glory of the forest.

June 21, 1819.

13, Tavistock-street, Bedford-square.

VIII. *On an old Method of marking Dates on Manuscript Books.* By GAVIN INGLIS, Esq.

To Mr. Tilloch.

DEAR SIR, —SOME time ago a collector of rare and curious volumes brought me an old manuscript Prayer Book, beautifully written on vellum, and partially illuminated, and requested I would do him the favour to look it over and see if I could trace any thing like a date, or mark by which its age might be ascertained, which

which he said constituted its chief worth, and would stamp it with a greater or less value in proportion to its antiquity. He had shown it to different clergymen, who severally examined its contents, without being able to affix any period, or observing any mark, or character, by which they could trace the date of its origin, although the year was very pointedly dated at the close of the calendar, but in a manner somewhat characteristic of the age of the church to which it belonged, when every thing was concealed under some mystic symbol, or almost unintelligible hieroglyphical hyperbole.

As the date of this little volume escaped the scrutinizing observation of these learned gentlemen, the same mode of dating may have been adopted in other writings of the same period, and perhaps of much greater importance, and which may also have escaped the researcher's notice, when its identity might have been of some consequence. Perhaps the decyphering of this may give a key, and lead to others of more interesting import. On this ground, may I trouble you to give it a few minutes' consideration, and if you judge it at all likely to be useful or amusing, have the goodness to give it a corner in your next number of the Philosophical Magazine?

The calendar was written in the regular style, the manuscript of which had been finished on the first day of the second month, one thousand four hundred and fifty-six, veiled under the following mystical dotting, made at the close of the last month in the calendar.

. . . : . . . : . . .
 . . . : . . . : . . .

To render it the more obscure, there was neither point nor comma, to divide the numbers, which would have made it somewhat plainer, thus:

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The single dot is the *first day*, the two horizontal dots mark *two*, for the second month. The three dropping or perpendicular dots, in imitation of the old I are only a numerical *one*. The *quatre four*, the *cinq five*, and the *six* by being laid horizontally denotes the currency of its numbers, and is to be counted

1 2 3
 : : : i. e. *six*. Had the six dots been placed thus : : they

4 5 6
 would have only counted *two*: had the date been fifty-three, the

three must have stood thus : : : or thus . . . , four would have

have been a repetition of the quatre, &c. Hence the date of the book must be 1st of the 2d month 1456 Ann. Dom.

Yours truly,

Strathendry, June 21, 1819.

GAVIN INGLIS.

IX. *Experiments for a new Theory of Vision.* By JOSEPH READE, M.D.

To Mr. Tillock.

Καλλίστα ἰδεῖν. Xen.

SIR, — PERHAPS no subject in natural philosophy has more engaged the attention of the learned, or claimed more interest, than that concerning the proper seat of vision. For two hundred years the retinal theory has been maintained, and its difficulties, if not absurdities, softened down by the learned ingenuity of mathematicians and metaphysicians. Well aware that to overturn a theory so universally adopted, and stamped with the seal of antiquity, requires a number of well regulated experiments and legitimate deductions, I now commit my endeavours to the candour of the intelligent reader.

Exp. 1st.—Having often remarked, when examining the eyes of patients, that surrounding objects, such as a lighted candle, &c. were painted on the transparent cornea in a beautiful and minute manner, as on the face of a convex speculum, it occurred to me that the mind might receive impressions or ideas from those erect images; and I was the more desirous of bringing this interesting suggestion to the test of experiment, in consequence of the many difficulties attached to the present system of vision. I now pasted two narrow strips of black cloth in the shape of the letter T, and about three inches in length, on one of the upper panes of a large and well-lighted window. I then requested a gentleman with a large pupil and good sight to seat himself about four or five feet from the latter, and to fix his eyes steadily on it. Looking into his pupil I perceived the letter T to be minutely yet distinctly painted by reflexion. I then took a plano-convex lens in my right hand, such as school-boys use for burning-glasses, and held it close to the pupil. On again looking at the corneal image of the letter T, I perceived it enlarged or magnified in all its dimensions, and the spectator said, he also perceived it much larger than with the naked eye. On removing the lens a little further from his eye, I perceived the letter on the pupil not only magnified, but surrounded with colours; and now the spectator saw the letter large, confused, and surrounded with colours. So far the phenomena of vision answered exactly to the

the changes of this corneal image. I next removed the lens somewhat further from the eye, and on looking into it perceived the letter T to be inverted, and the spectator likewise saw it inverted. He now took the lens in his own hand, and placing it at different distances before his eye, I was enabled by means of the corneal image to tell him what he saw. Having again requested the spectator to fix his eyes on the letter, I placed a concavo-concave lens before the pupil, and the letter was immediately diminished; he said he now saw it very small. Here I shall beg leave to remark that these experiments strike at the very first principles as laid down for optical instruments. For we find by these two simple and conclusive experiments, that a convex lens, instead of converging the rays as first maintained by Maurolycus in his treatise *De Lumine et Umbra*, actually and *bond fide* diverges and magnifies the images in all its dimensions; and on the other hand, that a concave glass converges or diminishes the image. The object of this paper being merely to draw the attention of the scientific to my opinions on optics, and particularly on vision, I shall not at present enter more fully into the theory of spectacles, object-glasses, &c.

Exp. 2d.—Having placed a plano-convex lens at such a distance before the spectator's eye, as to form an inverted image of the letter T on his pupil, I placed a concavo-concave lens behind, so as to represent an opera-glass or Galilean telescope. The inverted corneal image immediately became erect, and the spectator said he also saw it erect.

Exp. 3d.—The above experiments were made at about four feet from the window. I now requested the spectator to remove his chair to within a foot of the object; and on placing a convex lens immediately before the eye, the corneal image was considerably magnified: on slowly removing the lens more to the letter, and further from the eye, the black corneal image began to be surrounded with colours; but did not become inverted, nor did the spectator perceive any change of position; when close to the object the corneal image appeared better defined and more distinct. I next placed a prism before his eye, and desired him to look through the lower refracting angle. As he was unaccustomed to the application of this instrument, he could not regulate it so as to perceive the coloured image of the letter T. I therefore turned the prism until I perceived it on the pupil, and then told him exactly what he saw, making a mirror of his eye. Let us now inquire what changes the intervention of a plano-convex or a concave glass would make on the letter T brought to a focus on the retina by means of the crystalline and other humours. Having removed the fat and coats from the back part of an ox's eye, as performed by Kepler and Scheiner, and thus laid bare the retina,

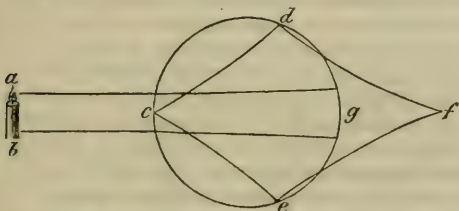
I placed a lighted candle in front. An inverted image was seen as if floating on the retina. I now placed a plano-convex lens between the candle and the cornea, at such a distance as to form an inverted image on the pupil. The retinal image remained inverted. On now placing a concavo-concave lens at a little distance before the convex one, the corneal and inverted image became erect, while the retinal inverted image was not in the least changed as to position. Here is a direct experimental proof, that even if an inverted image were painted on the retina, that inverted image, not undergoing any change of position by the intervention of the glasses, could not be the image conveyed to the mind. To suppose for one moment, that an inverted image on the retina could produce both the idea of inversion and erection, would be adding another inconsistency to Kepler's catalogue. In this experiment the changes of the corneal image were accompanied by simultaneous changes in the mind; therefore that, and that alone, must have produced the sensation. On placing a glass globe about two inches diameter filled with water opposite the letter T on the window, and then interposing a convex lens, the posterior inverted image was obliterated, the rays of black light not being sufficiently strong. The same thing took place with a concave lens. Dr. Priestley, who wrote a number of metaphysical works, gravely informs his readers, "that the want of an inverted image might produce the sensation of an erect one." With great respect for the Doctor's opinions, we might just as readily believe that the want of a man's dinner would get him a supper!

There is no inverted image ever painted on the retina.

Having removed the fat and coats from the back part of an ox's eye, and thus bared the retina, in imitation of Kepler's and Scheiner's experiments, I placed a lighted candle on a table in front; and on looking through the retina, my eye being placed beyond the principal focus of the sphere (or rather two segments of one), I certainly did perceive a beautiful and inverted image of the candle as if floating on the retina. So far the experiment seemed to accord with the retinal theory of vision; for, if the rays were refracted and converged, as represented by optical writers, by means of the cornea, aqueous humour, crystalline lens, and vitreous humour, they should cross nearly in the centre of the eye, and finally paint an inverted image on the retina. However, on approaching my eye nearer to the retina, I perceived the inverted image to become large, confused; and when my eye was very close, it opened into two curved and inverted images, which receded laterally, and at a yet nearer approach formed into a circle, through the centre of which I perceived a very distinct and erect image of the candle, evidently coming from the anterior surface of
the

the eye, and perfectly distinct from the inverted one, considerably magnified in passing through the humour. Kepler, in placing his eye beyond the focus of the ox's eye, which is nothing more than a simple sphere, saw an inverted image formed by the junction of the two images painted on his own cornea, which he mistook for one on the retina, as a person looking into a concave mirror thinks he sees an inverted image in the glass. Here I think it necessary to give a rough sketch of the passage of the rays through the eye, particularly as my opinions are diametrically opposite to those of all optical writers.

A glass globe filled with water, and about two inches diameter, may serve those unacquainted with morbid dissections: $a b$ are two



rays of light coming from the upper and lower parts of the candle, impinge on the transparent cornea at c , and paint an erect image. This image again transmits rays, diverging as they pass through the sphere to g , where the spectator sees a magnified and erect image. The image at the cornea c also sends rays, forming inverted images in consequence of the rays crossing;—these images take the curvature of the glass globe, and, uniting into one inverted image, form what has been denominated the principal refracted focus at f . Now it is evident that Kepler, to have made his experiments correctly, should have placed his eye at g , and not at f ; indeed, his eye should almost touch the retina; and then, as I have already said, he would have seen an erect magnified image of the candle, and not an inverted one surrounded by what optical writers denominate a circle of aberration. It is really surprising how any person could for a moment believe that this circle of aberration could produce vision, according to the present theory of vision. Long-sightedness they say is produced by the image being formed beyond the retina. Short-sightedness, by an image formed before the retina in the vitreous humour,—both physical impossibilities! The rays of light are supposed to be converged in the body of the eye. I would beg leave to put the following question: Would the crystalline lens, when embedded in the vitreous humour, act in the same manner as it would in air? Certainly not, as the following easy experiment may show. Take a large basin of water, holding a powerful glass lens in such a position over the water as to form an inverted image

on the side of the basin and under the water ; then immerge the entire lens, and at no distance whatsoever can a focus be ever formed. The circular shadow with its black circumference is perceived, but nothing else. Now surely the refractive power of glass, in proportion to that of water, is much greater than that of the crystalline lens in proportion to that of the vitreous humour. Mr. Harris in his *Optics*, p. 95, says: " It is very difficult, I think, to determine accurately the measure of these refraction ; but, from such experiments as could be made, it has been found that the refractive powers of the aqueous and vitreous humours are each of them much the same with that of common water, and that of the crystalline lens is a little greater : that is, the proportion between the sines of incidence and refraction out of air into the cornea or aqueous humour, is as 4 to 3 : out of the aqueous humour into the crystalline, as 13 to 12, and out of the crystalline humour into the vitreous, as 12 to 13." From analogy we are authorized to conclude, that the crystalline lens can never form an inverted image on the retina, and that the lens is placed in the centre of the eye to magnify or diverge the rays, and not to converge and invert the object. Moreover, that the crystalline lens does not produce inverted images on the retina, is shown by what takes place when removed by the operation of depression or extraction. For, if the lens were so essentially necessary to vision, its removal must cause blindness. In answer to this objection it has been stated, that after the operation the patient is obliged to use convex glasses or spectacles to supply the place of the lens. From many years practice in those complaints, I am enabled to say that this is by no means the case. In young patients, the use of convex glasses, although at first of assistance, is ultimately unnecessary, if not injurious, as the eye gains strength, and is enabled to see all objects at a limited distance fully as well as those labouring under short-sightedness. Some time since I removed a congenital cataract from the right eye of Mary Skillington aged 19. After the operation she never wore a glass, and can now see to thread a needle ; she also sees perfectly well at different distances to the extent of 200 feet and upwards. Miss Jenkins of Bantry writes and reads perfectly well, attending to the business of her shop, without the use of spectacles. This lady came to Cork to consult a London quack, who professed to cure all diseases of the eye that were curable : luckily she did not come under that denomination. Indeed, after the operation in young subjects, I never recommend the use of a convex glass. In those patients wanting the crystalline lens, the rays cannot come to a focus on the retina. Yet had Kepler and Scheiner removed the lens from the ox's eye, as I have repeatedly done, before the experiment, they would have found that it made not the slightest difference

difference in the inverted image, which they conceived to float on the retina; neither could the crystalline jump backwards and forwards to accommodate the eye to the object at different distances. Indeed I cannot conceive the cause of this jumping of the lens. If the distance of the object be ascertained, and consequently the object seen before the lens with its thousands and tens of thousands of muscles begins to jump, what occasion is there for that movement? But if the jump take place before the object is seen, how can the extent of the jump be ascertained? Look before you leap, is a common but a necessary piece of advice. The fact is, that the eye principally judges of different distances, by comparing the visible size of the corneal image with the educated sense of the tangible object, intervening objects, strength of colouring, &c. In viewing a painting, the objects are all equidistant on the canvass; yet we conceive them to be at relative distances. The following easy experiment may also show that the rays diverge in passing through a sphere or convex glass. Take a cylindrical tumbler, fill it with clear water, holding it in the left hand opposite a window. Then hold a black slate pencil, or any other slender body, about three inches in length, behind this glass vessel. When close, one magnified image is seen; but on gently withdrawing the pencil to a greater distance, this image becomes more magnified; and at a certain distance, two images fully as well defined are seen at each side of the tumbler. On continuing to withdraw the pencil, these two everted images are seen to glide with a considerable degree of curvature towards the posterior surface of the tumbler, and at last coalescing into one image, obliterate the anterior one, or that formed at the anterior surface. This corroborates the inferences drawn from the former experiment. When the object is near the posterior surface of the tumbler, the eye receives the rays considerably magnified or diverged; but when the object is at some distance from the posterior surface, the eye receives the rays from the united lateral images. From this experiment, there can be no doubt whatsoever that the eye receives rays from two distinct and separate images, and also that the mind receives impressions from a glass globe, or convex lens, in nearly a similar manner.

Should a doubt yet remain, the following experiment may be made. Place a red wafer under one of the planes of a triangular glass prism, resting on a sheet of white paper on the table, we immediately see two everted images of the wafer formed in

each lateral plane, as thus represented:  The wafer *a*

sends rays or images to *b* and *c*. As the prism has plane sides, the two images can never come to a focus at any distance from the eye; but if we round off the angle, they immediately unite,

and form an oblong image of the wafer, as thus represented:



. From these experiments, and many others hereafter to be related in the 2d volume of the Experimental Outlines, not a doubt remained on my mind, that reflected erect and not inverted images gave mental impressions of a visible world. Surely, if any thing can increase our admiration of the power and wisdom of a supreme Being, it is the conviction that a beautiful and ever-varying landscape is painted in miniature on the transparent cornea. When we consider that the black choroid shines through the retina, we must admit that it is very unfit to be the reflecting mirror of the mind. To bring this to the test of experiment, I turned out the aqueous, vitreous and crystalline humours of an ox's eye. On bringing the inverted image of the black letter T, pasted on the window, by means of a convex lens, to float on the retina, I found that in some places it was perfectly invisible, in other places confused, indistinct in all. Indeed the retina, were it free from this and many other objections, and also free from the large blood-vessels and nerves running over its surface, being of a gray colour, like pounded glass or animal jelly, would be very unfit, and could never form an image of a gray object perfectly similar to itself: neither could objects the colour of the choroid coat ever be seen. We might as well think of writing with black ink on a sheet of black paper, as attempt the formation of dark images on a dark ground. On the other hand, how admirably fitted both for reflexion and transmission is the cornea! both sufficiently transparent and sufficiently opaque: no coloured substance could answer the purpose. It has hitherto been the received opinion, that the two optic axes, concurring at the object, make an angle according to the size of which the object appears large or small;—but this opinion, whose inconsistency has already been pointed out by Bishop Berkeley, must yield to the more rational theory that the mind takes the apparent magnitude and distance from the size of the corneal image, and not from lines and angles beyond the nervous influence, or from invisible rays; all rays being invisible which are transparent until intercepted and reflected. “In vain (says Berkeley) shall all the mathematicians in the world tell me that I perceive certain lines and angles which introduce into my mind the various ideas of distance, so long as I myself am conscious of no such thing.” Indeed we might as well believe in ghosts and hobgoblins, as to believe that we would see an object, or the image of an object, beyond the nerves, that is, beyond the transparent cornea. Here lies the Rubicon, the utmost limit, beyond which the mind can never travel. Surrounding objects are brought to the eye, by means of the solar rays of light; hence the nerves convey them to the sensorium.

rium. Indeed, the idea that the mind or sense could travel beyond the cornea, ride on the whirlwind, and, like a fairy Mab, measure invisible angles of an invisible and distant image, is so very inconsistent, that we cannot but express surprise at its adoption.

If a man were gravely to say that he could touch the moon, he would be looked on as mad: but an astronomer says, that on looking through a telescope he can measure the invisible image of that body nearer to the eye than the moon, and beyond the influence of the nerves, and the astronomer gets credit for the assertion. As the knowledge of distance almost entirely arises from experience founded on the analogy between the sense of sight and touch, the former at a very early period of existence is inadequate to regulate our perceptions. When an infant begins to notice, natural education commences, external objects are the letters, the nerves the instructors of the mind: the insufficiency of sight is evident by the anxious desire to feel and to examine every new play-thing; the image of the rattle is delineated on the cornea, and the child, believing it to touch the eye, grasps at it although far removed. On the same principle, I have heard a child cry bitterly for the moon to play with. In a few months the sense of touch has partly educated the eye in judging distance by the apparent magnitude of the corneal image. A man born blind and suddenly restored to sight would suppose every object to touch his eye.

All that is accomplished by telescopes and microscopes (according to the retinal theory) is first to make an image of a distant object by means of a lens, and then to give the eye some assistance for viewing that image as near as possible, so that the angle which it shall subtend at the eye may be very large compared with the angle which the object itself would subtend in the same situation:—this is done by means of an eye-glass, which so refracts the pencils of rays, as that they may afterwards be brought to their several foci by the natural humours of the eye. Now it is evident from the foregoing experiments that this theory is fallacious, and that a telescope, as shall hereafter be more fully shown, does nothing more than diverge the rays, or magnify the image on the cornea. In the Galilean telescope the convex lens magnifies the erect image which it forms on the concave eye-glass, the use of which, by regulating the sphere of concavity, is to obviate the colours produced by the sphere of convexity: hence an achromatic and magnified corneal image is formed.

I shall here notice a difficulty which Dr. Barrow and all other opticians have failed to clear up, particularly noticed by the Bishop of Cloyne: “Let an object be placed beyond the focus of a convex lens, and if the eye be close to the lens it will appear

confused, but very near to its true place. If the eye be a little withdrawn the confusion will increase, and the object will seem to come nearer; and when the eye is very near the focus, the confusion will be exceedingly great, and the object will seem to be close to the eye. But in this experiment the eye receives no rays but those that are converging, and the point from which they issue is so far from being nearer to the object, that it is beyond it, notwithstanding which the object is conceived to be much nearer than it is, though no very distinct idea can be formed of its precise distance." Here Dr. Barrow supposed that when his eye was close to the lens it received none but converging rays, whereas they were diverging, and as he withdrew his eye, the more the erect image was magnified; when magnified beyond the standard of distinct vision, it became confused; but when the eye was beyond the focus, the anterior or erect image was lost to the eye, and the two lateral and inverted images united into one, forming an image nearer the eye, as if floating on the posterior surface of the lens. Dr. Barrow, like a true philosopher, acknowledges himself unable to account for this appearance, finishing his lecture with this observation: "*Vobis itaque nodum hunc, utinam felicior conatu, resolvendum committo.*" Whether these experiments tend to untying the knot, I leave the reader to determine, and shall not enter on Berkeley's or Barrow's theories of apparent distance in this paper.

We now come to the rectification of inverted images on the retina. This according to Scheiner and Kepler is the business of the mind, which, when it perceives an impression on the lower part of the retina, considers it as made by rays proceeding from the higher parts of the object tracing the rays back to the pupil, where they cross one another. But this hypothesis, says Dr. Priestley, will hardly be deemed satisfactory; and by way of clearing up the difficulty he proceeds: This "upper and lower are only relative terms; and as all objects are painted upon the retina in a similar manner, all the upper parts in one direction, and all the lower parts in another, it is by custom only, founded on experience and the association of ideas, that we learn to distinguish them from one another, so as to direct our eyes or point our hands upwards or downwards as we have occasion. If this be the true solution (continues the learned Doctor), it will follow, that if the images of objects had always been painted in a different manner—that is, erect as the objects themselves are—we should have acted as we do now without being sensible of the difference, a different association of ideas only having taken place." Now all this laboured explanation comes to nothing more or less than that we are taught by experience. However, we never find the infant or the brute (both incapable

capable of these refined associations) mistaking the top for the bottom, or the right for the left. When the world was turned upside down by philosophers, they should have attributed the circumstance to blind instinct, and not to reason: indeed, reason has nothing whatsoever to do with the business. In the summer of 1812 I performed the operation for cataract on a very intelligent boy, named Edward Casay, aged ten years. He was born with such opaque cataracts (according to his mother's account) as merely to enable him to distinguish light from darkness, or the shadow of an interposed hand, but was incapable of distinguishing the outlines of any object or the most brilliant colours. After the operation, and before he could acquire any ideas from association, having inquired the manner in which he saw; he answered that he saw objects as he felt them, supposing them to be very near the eyes. Although Cheselden was an advocate for the retina being the seat of vision, he does not make any particular observations on this difficulty; but says that the young gentleman whom he couched with congenital cataracts "knew not the shape of any object, nor any thing from another, however different in shape or magnitude; but upon being told what things were, whose form he before knew from feeling, he would carefully observe that he might know them again." When shown his father's picture, and told what it was, he acknowledged a likeness, but did not mistake the head for the feet. Indeed, were there no other difficulty in the retinal theory of vision, the inversion of objects, or turning the world upside down, and making confusion right and left, would be sufficient to invalidate the entire. The next difficulty in this catalogue of difficulties is the power of seeing objects distinctly at different distances. It is allowed on all hands, that to see an object at different distances, either the retina or crystalline lens must approach, so as to shorten what is called the optic axis. So that the crystalline, according to the retinal theory, must be a great and incessant jumper. To calculate the number of jumps or little leaps the lens of a general officer would take at a review, might puzzle the most able algebraist; vulgar arithmetic would be inadequate to the solution; and then an able philosopher has given thousands and tens of thousands of muscles (or wings if you please to call them so) to this little busy fluttering thing. Indeed, the justly celebrated Dr. Young might as well have given muscles to an onion, the lamellæ of which and those of the crystalline are very similar. On examining the different theories on this subject, we find them all to differ, and perfectly inadequate to the effect. Kepler two centuries ago supposed that the contracting of the ciliary processes draws the sides of the eye towards the crystalline; by which means the eye is lengthened, and the re-

tina

tina pushed to a greater distance from the pupil when we are viewing near objects. Dr. Thomas Young differs from Kepler; Descartes from both; Haller from all: with a crowd of others whose opinions I think it unnecessary to mention.

I remain, sir, your obedient servant,

Cork, April 1, 1819.

JOSEPH READE, M.D.

X. Observations on the Means of preserving Provisions and Goods. By JOSEPH MACSWEENEY, M.D.

To Mr. Tilloch.

THE plan for preserving provisions, which I intend to suggest in this paper, promises to be useful on account of the facility with which it may be put in practice. If proposed before, it has not excited attention, while more complicated and more expensive processes have been attended to. I hope that this paper will be the means of drawing attention to a matter of such consequence to the public, particularly when it is in the power of every house-keeper to put the plan into execution. I had determined a considerable time ago to commence experiments to try to preserve meat by keeping it in water (previously boiled) covered with a layer of oil to protect it from the agency of the atmosphere. After reading the interesting experiments of Dr. Marshal Hall, I resolved to add bright pieces of iron or iron-filings to the water, for the purpose of depriving it of the oxygen not expelled by boiling. It is now more than seven weeks since I put some fresh meat with iron in water, boiled and covered with a thick layer of oil. The meat has exactly the same appearance now that it had the first day. It may be necessary to add that I kept the glass-vessel containing it in a dark place. Vegetable substances may be preserved in the same way. Delicate substances of this kind when kept in a dark place appear only to suffer from the solvent power of the water*. In the same way cutlery may be kept untarnished, when the precaution is taken of adding some iron-filings or bright pieces of iron to the water some time beforehand. After twenty-six days, I removed a piece of iron from a solution of common salt covered with oil, and found it not tarnished in any degree. Goods of every kind may be preserved from decay on the same principle. Lest any, such as linen or cotton goods, might suffer from the iron, it should be put in small timber boxes and let down through the oil into the water, so that it could abstract the oxygen without coming in contact with the goods.

* The solvent power of the water may be diminished by adding sugar and gum.

When

When it is necessary to remove meat for use, it can be taken out of the vessel without allowing it to come in contact with the oil. By pouring in water, the oil will ascend and will flow over the edge of the vessel containing the meat into one placed underneath to receive it; thus the meat can be easily removed, and the oil will not be wasted. Fresh water can be preserved in like manner in situations where it is scarce, by covering its surface with oil, and by putting bright pieces of iron in it.

London, July 6, 1819.

XI. On Dr. CARTWRIGHT's *Pedo-motive Machine*. By Mr. HENRY MEIKLE.

To Mr. Tilloch.

London, July 5, 1819.

SIR,—DR. CARTWRIGHT, in giving an account of his invention, in your last Number, says “that it seems nothing less than a miracle, that the idea did not occur to the common knife-grinder centuries ago.” If I do not mistake the thing altogether, Hardie's walking-crane is nothing else than the same principle brought to great perfection; and if I rightly remember, there is in the *Encyclopædia Britannica* a description with an engraving of a carriage moved with ratchet-wheels and treddles, which I conceive to be precisely on the same principle as that proposed by the Doctor at the bottom of page 425. However, as the account which Dr. C. gives of his invention is very concise, he himself can best explain his meaning if I have misconstrued it.

I am, sir,

Your most obedient servant,

HENRY MEIKLE.

* * In laying the above observations of our correspondent Mr. Meikle before our readers, we beg to offer one remark. The idea of working a wheel-carriage by means of ratchet-wheels moved by treddles is not new; but we have no recollection of having before seen any proposal for applying the whole muscular strength of a man's body, by means of straps over his shoulders, to the working of the treddles. This is what we consider as the principal novelty in Dr. Cartwright's suggestion, and that which alone gives it value as an improvement. If the strap or any fixed point of resistance for the man's shoulders, while his feet are employed to communicate his power, has been before in use, the Doctor's improvement is not new.—EDIT.

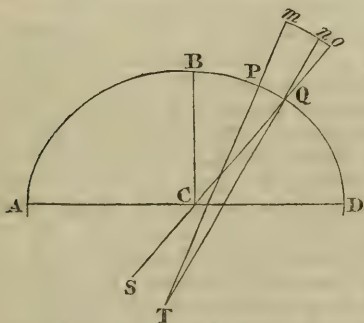
XII. *Observations on the Measurement of an Arc of the Meridian.* By T. FIRMINGER, LL.D.*

THE measured meridional arcs, in different degrees of latitude, have not been found to give a ratio of the earth's axis at all consistent either with the theory of a homogeneous ellipsoid of revolution, or with themselves, as deduced by trials of measured arcs in different latitudes : for while some give a ratio nearly the same as Newton has assigned it from theory, others, on the contrary, make the polar axis the longest, or the earth flattened at the equator. Many causes have been assigned as the probable reason of the discrepancy in the results. Colonel Mudge and Captain Kater have attributed the want of agreement to the effect of local attraction : but they differ *in toto* as to the effect of its influence ; or rather, as to the situation of the attracting mass. Colonel Mudge believes the principal effect to take place at Clifton, and Captain Kater at Dunnose. Others again have doubted whether the amplitude of the celestial arc has been correctly obtained ; and have supposed, either that the zenith sector with which Colonel Mudge made his observations, might not have been exactly placed in the plane of the meridian, or that some error has been committed in the use of it. Mr. Fisher, in a paper just published in the Quarterly Journal of Science and the Arts, endeavours to account for and to rectify these discrepancies, by a supposition "that measured arcs of the meridian are not legitimate measures of the radii of curvature" at the middle point of curvature of those arcs. Mr. Fisher seems to have been led into this idea from the consideration of the position of a plumb-line upon a sphere having the same rotation as the earth urged by a centrifugal force : and having calculated the deflections from the centre, at each degree of latitude, he found that the difference, in the deflections, upon the whole arc measured by Colonel Mudge, amounted to exactly that quantity which would reduce the arc to agree with an excentricity of 229 to 230 ; from which he concluded that measured arcs are not legitimate measures of the radii of curvature, and "that so long as these measured arcs are considered arcs of circles, this correction for deflection will obtain. Mr. Fisher was no doubt led into this mistake from finding that his differences rectified the arcs in those instances in which he applied them ; and too hastily adopted his conclusion, without taking time to examine and consider the problem : for although the arc of a circle and ellipse of the same number of degrees, and of the same radius of curvature, will differ in length ; yet this difference is too small (as Dr. Hutton observes, in the account of the formula alluded to by Mr. Fisher,) to sensibly affect the result. There is a circumstance however which has not, that I

* Communicated by the Author.

know of, been noticed by any mathematician, and which will operate, if it amounts to any quantity, as a correction to the observed meridional arc. In an ellipse no two points have the same radius of curvature in the same quadrant: taking, therefore, any part of a meridional line upon the earth's surface (as for instance from Dunnose to Clifton), and conceiving this as an arc of an ellipse, the radii of curvature, at each extremity of the arc, will not have the same centre; and, therefore, the difference of the zenith distances of any star, taken at the two extremities, will not be a correct measure of the elliptic arc, or rather of an arc of a circle of curvature, which the formula requires.

Let ABD be a semi-ellipse, PQ an arc; let PT, QS be two



lines drawn perpendicular at P and Q, and let TP be a radius of curvature at P, and let TQn be drawn: m and o are the zenith points at P and Q, supposing PQ to be an arc of the meridian upon the earth's surface; but m is the measure of PQ, which the problem requires; and therefore an arc in the heavens, measured by zenith distances, is not a correct measure of a corresponding arc of curvature on the earth's surface. In the arc measured by Colonel Mudge's sector between Dunnose and Clifton, the angle nQo , I believe, will be about eight seconds of a degree. If therefore all the stations of Colonel Mudge should be verified by independent observations, it will not show the result deduced by him to be correct, as the meridional difference of zenith distances of any two places upon the earth's surface, is accurately the difference of latitude of those places. If the angle nQo should, upon accurate computation, amount to $8''$ upon the whole arc measured between Dunnose and Clifton, the arc between Dunnose and Arbury Hill will require a correction of about $3''5$, leaving the results deduced by Colonel Mudge as inconclusive as before, as to the elliptical figure of the meridian. Would it not, therefore, be useful to determine the latitude of the

the several stations lying near the direction of the meridian with a good repeating circle, or other astronomical instrument of great accuracy, upon circumpolar stars above and below the pole? as such observations would remove all doubt upon the measure of the celestial arcs, and might be soon made by any zealous and active observer.*

XIII. *Notices respecting New Books.*

Transactions of the Geological Society, Vol. V. Part I. 4to.
pp. 309.

THE following are the contents of the present addition to the valuable *Transactions* of this Society:

- I. On the Island of Salsette. By Stephen Babington, Esq.—
- II. Remarks on the Hills of Badaeson, Szigliget, &c. in Hungary. By Richard Bright, M.D.—
- III. Some Observations on a Series of Specimens presented to the Geological Society by the Hon. H. Grey Bennet. By Arthur Aikin, Esq.—
- IV. Remarks on the Chalk Cliffs in the Neighbourhood of Dover and on the Blue Marle covering the Green Sand near Folkstone, with an Appendix containing some Account of the Chalk Cliffs, &c. on the Coast of France opposite to Dover. By William Phillips, Esq.—
- V. Remarks on the Fossils collected by Mr. William Phillips near Dover and Folkstone. By James Parkinson, Esq.—
- VI. Notes accompanying a Set of Specimens from the Himalay Mountains. By James Fraser, Esq. of Calcutta.—
- VII. Observations on the Valleys and Watercourses of Shropshire and of Parts of the adjacent Counties. By Arthur Aikin, Esq.—
- VIII. On the Form of the Integrant Molecule of Carbonate of Lime. By Dr. Brewster.—
- IX. Description of some new Fossil Encrini and Pentacrini lately discovered in the Neighbourhood of Bristol. By George Cumberland, Esq.—
- X. On the Limestone Beds on the River Avon near Bristol; with a Description of the Magnesian Beds that repose on their Basset Edges. By George Cumberland, Esq.—
- XI. On the Strata of the Northern Division of Cambridgeshire. By Francis Lunn, Esq.—
- XII. Memoir on the Geological Relations of the East of Ireland. By Thomas Weaver, Esq.—
- XIII. On the Modifications of the Primitive Crystal of the Sulphate of Barytes. By William Phillips, Esq.

* If there should be found at the station at Arbury Hill an error in the measure of the celestial arc amounting to 3 or 4 seconds of a degree, which from the smallness of the quantity may have crept into the account, and also if the angle nQo upon the whole arc is found by accurate computation to amount to about $8''$, the whole will agree with an elliptical meridian of 229-230 very nearly, and also agree with deductions derived from measurements upon other parts of the earth.

A Critical Examination of the first Principles of Geology, in a Series of Essays. By G. B. GREENOUGH, President of the Geological Society, F.R.S. F.L.S. 8vo. pp. 336.

The essays in this volume, which are eight in number, treat of Stratification—of the Figure of the Earth—of the Inequalities which existed on the Surface of the Earth previously to the Diluvian Action, and on the Causes of these Inequalities—of Formations—of the Order of Succession in Rocks—of the Properties of Rocks as connected with their respective Ages—of the History of Strata as deduced from their Fossil Contents—and, of Mineral Veins.

Transactions of the Royal Geological Society of Cornwall.
Illustrated by Plates. Vol. I.

The Royal Geological Society of Cornwall was instituted on the 11th of February, 1814. The present, which is the first volume of its Transactions, contains the following articles:

I. On a recent Formation of Sandstone occurring in various Parts of the Northern Coast of Cornwall. By John Ayrton Paris, M.D.—II. An Account of some Granite Veins at Porth Just, near Cape Cornwall. By John Davy, M.D.—III. Notes on the Coast West of Penzance, and on the Structure of the Scilly Islands. By Ashhurst Majendie, Esq.—IV. A Sketch of the Geology of the Lizard District. By Ashhurst Majendie, Esq.—V. Hints on the Geology of Cornwall. By Sir Humphry Davy, LL.D.—VI. Observations on a remarkable Change which Metallic Tin undergoes under peculiar Circumstances, and on its partial Conversion into a Muriate of Tin. By the Rev. William Gregor.—VII. Observations on the Process for making the different Preparations of Arsenic, which are practised in Saxony, and on those for preparing Smalts or Cobalt, as pursued in Bohemia. By John Henry Vivian, Esq.—VIII. A Sketch of the Plan of the Mining Academies of Freyberg and Schemnitz. By John Henry Vivian, Esq.—IX. On the Accidents which occur in the Mines of Cornwall, in consequence of the premature Explosion of Gunpowder in blasting Rocks, and on the Methods to be adopted for preventing it, by the Introduction of a Safety Bar, and an Instrument termed the Shifting Cartridge. By John Ayrton Paris, M.D.—X. On Elvan Courses. By Joseph Carne, Esq.—XI. Notes on the Limestone Rocks in the Parish of Veryan. By S. J. Trist, Esq. With an Appendix by the Rev. John Rogers.—XII. On the Discovery of Silver in the Mines of Cornwall. By Joseph Carne, Esq.—XIII. On Submarine Mines. By John Hawkins, Esq.—XIV. On some remarkable Phenomena attending the Lodes of Polgooth Tin Mine. By John Hawkins, Esq.—XV. Remarks on the Salt Mines of Wielitska in Poland, and of Salzburg in Germany. By John Henry Vivian, Esq.—XVI. Observations on the Geological Structure

Structure of Cornwall, with a View to trace its Connexion with, and Influence upon, its Agricultural Œconomy, and to establish a rational System of Improvement by the scientific Application of Mineral Manure. By John Ayrton Paris, M.D.—XVII. On a Process of Refining Tin. By John Hawkins, Esq.—XVIII. On the Introduction of the Steam-Engine to the Peruvian Mines. By Henry Boase, Esq.—XIX. Miscellaneous Notices of Facts connected with the Mineralogical, Geological, and Œconomical History of the County of Cornwall. Extracted from the Minute Book of the Society, viz. 1. On Kupfer-Nickel. By the Rev. William Gregor.—2. On a Cavern in Dolcoath Mine. By John Rule, Esq.—3. On Stalactites. By A. Majendie, Esq.—4. Gregorite (Menacchanite) discovered at Lanarth. By John Ayrton Paris, M.D.—5. On the Formation of a Substance resembling Gneiss, taken from a Steam Boiler at Huel Alfred. By J. A. Paris, M.D.—6. A new Substance accompanying “Welsh Culm.” By J. A. Paris.—7. On Stones and Clays annually exported from Cornwall, for the Purposes of Architecture, Manufactures, and the Arts. By J. A. Paris.—8. Observations on Gold found in the Tin Stream Works of Cornwall. By Sir Christopher Hawkins, Bart.—9. Vegetable Remains in the Basin at Portleven. By the Rev. John Rogers.—10. Contributions towards a Knowledge of the Geological History of Wood Tin. By Ashhurst Majendie, Esq.—11. Notice relative to the Formation of Swimming Stone. By Joseph Carne, Esq.—12. Notice on Elvan Courses. By Joseph Carne, Esq.—Queries proposed to Captains of Mines, and other Persons connected with the practical Part of Mining. By John Hawkins, Esq.—Queries respecting Lodes. By John Hawkins, Esq.—Account of Tin raised in Cornwall in the Years ending with Michaelmas Coinage 1815, 1816, and 1817.—Account of the Produce of all the Copper Mines in Cornwall, in Ore, Copper, and Money, for the Years ending 30th June 1815, 1816, and 1817.—Quantity of Copper produced in Great Britain and Ireland in the Years ending 30th June 1815, 1816, and 1817.

Conversations on Natural Philosophy, in which the Elements of that Science are explained and adapted to the Comprehension of young Pupils. Illustrated with Plates. By the Author of “*Conversations on Chemistry, and Conversations on Political Œconomy.*” 1819. 12mo. pp. 434.

The title of this little volume explains its object so fully as to render it unnecessary for us to say any thing on this point. We will only add, that the work is well adapted to the comprehension of those for whose benefit it is intended. The plates, which are twenty-three in number, are by Lowry—a sufficient passport for their accuracy.

General

General Views on the Application of Galvanism to Medical Purposes, principally in Cases of suspended Animation. By JOHN ALDINI, Honorary Member of the Royal Humane Society, Honorary Professor of the Imperial University of Wilna, &c. Svo, pp. 96.

This is an ingenious little work, written by Professor Aldini during his late visit to this country. It is inscribed to the Royal Humane Society, of which the author is one of the very few honorary members. He remarks truly in the dedication, that "in a commercial and maritime country like Great Britain, in which so many persons from their occupations at sea, on canals and rivers, and in mines, are exposed to drowning, suffocation, and other accidents; and it must be added, however painful the remark, where so many unhappy creatures labouring under a temporary privation of reason, form the horrible resolve of self-destruction—the subject of this work is of the utmost importance in a public view." With the intent of rendering the application of galvanism to the purposes of medicine generally, and to the cases of suspended animation in particular, more simple, Prof. A. has added to his observations on the nature of galvanism an account of some experiments which appear to us conclusively to demonstrate the power of galvanism over the organs of respiration.

Observations on the Medical Powers of Mineral Waters; containing an Account of the following most celebrated, viz. Malvern—Matlock—Bristol Hotwell—Buxton—Seltzer—Spa—Pymont—Bath—Tunbridge—Hastings—Cheltenham—Scarborough—Vichy—Carlsbad—Hartfell—Brighton—Sand- rocks—Seidlitz or Seydschutz—Epsom—Leamington—Sea—Harrogate—Moffatt—Aix-la-Chapelle or Aken—Bareges, &c. &c. and pointing out the Waters most beneficial in the Cure of different Diseases; with Remarks on the Cold Bath—Sea Bathing—Shower Bath—Tepid Bath—Warm Bath—Hot Bath—Vapour Bath—Nitro-Muriatic Acid Bath—and on sulphureous Baths. By PATRICK MACKENZIE, M.D.

A Report of the Practice of Midwifery at the Westminster General Dispensary during 1818; including new Classifications of Labours, Abortions, Female Complaints, and the Diseases of Children; with Computations on the Mortality among Lying-in Women, and Children; and the Probabilities of Abortion taking Place at different Periods of Pregnancy, &c. &c. With select Cases and Formulæ. By AUGUSTUS BOZZI GRANVILLE, M.D. F.R.S. F.L.S. M.R.I.

In the Press.

The Army Medical Officer's Manual, upon active Service; or, Precepts for his Guidance in the various Situations in which he
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may be placed; and for the Preservation of the Health of Armies upon Foreign Service. By J. G. V. MILLINGEN, M.D. Surgeon to His Majesty's Forces; Associate of the Medical Societies of the Faculty of Paris; and Corresponding Member of the Royal Medical Society of Bordeaux.

This publication will fully detail the various duties connected with the following important points:

Organization of the Medical Department of an Army—Formation of a Field Corps of Hospital Ambulance—Arrangement preparatory to the sailing of an Expedition—Duties during the Voyage—Disembarkation—Establishment of General and Convalescent Hospitals—Hospital for Sick and Wounded Officers—Depôts—Arrangements prior to taking the Field—Duties upon a March—Bivouacs—Encampments—Cantonments—Regimental and Brigade Hospitals—Duties during and after a Battle—Field Hospitals—Intermediate Hospitals, and Movement of the Sick and Wounded to the Rear—Duties with a besieging Force, or with a besieged Garrison—Duties upon a Retreat—Reinforcements moving to the Front—Re-embarkation of Troops in presence of an Enemy—General Conclusions and Recapitulation.

Part I. of *Pathological and Practical Remarks on Ulcerations of the Genital Organs, &c.* By JAMES EVANS, Surgeon of His Majesty's 57th Regiment of Foot.

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 22. A LETTER was read from Mr. John Freeman Wood to W. G. Maton, M.D., containing an account of a case of mal-conformation of the heart.

Another paper communicated by Dr. Thomas Young was read, entitled "Observations on the new System of diagonal Framing introduced by R. Seppings, Esq. into His Majesty's Navy;" by William Morgan, Esq.

May 6. — A paper by Dr. Brewster was read, On the optical and physical Properties of Tabasheer. The substance called Tabasheer, from the Persian *Scher*, or the Sanscrit *Kschiram*, signifying milk, has been long known in eastern countries, and formed an important article in the Materia Medica of the Arabian physicians. In the Gentoo language it is called *Bamboo Milk*; in the Malabar, *Salt of Bamboo*; and in the Warriar, *Bamboo Camphor*. It is found in the joints of the female bamboo, sometimes in a fluid state like milk, sometimes with the consistence of honey; but

but generally in the form of a hard concretion. Some specimens of it are transparent, and resemble very much small fragments of the artificial pastes made in imitation of opal ; others are exactly like chalk ; while a third kind is of an intermediate character, and has a slight degree of transparency.—The first person who examined the properties of this substance was Mr. Macie (now Mr. Smithson), who analysed a portion of the tabasheer from Hyderabad, which Dr. Russel had the preceding year presented to the Royal Society. “ From its indestructibility by fire—its total resistance to acids—its uniting by fusion with alkalies in certain proportions into a white opaque mass ; in others, into a transparent permanent glass ; and its being again separable from these compounds entirely unchanged by acids,” he considers it as perfectly identical with *common siliceous earth*. In the year 1804, Messrs. Humboldt and Bonpland brought with them from America some specimens of tabasheer called *Guaduas butter* by the Creoles, taken from the bamboos which grow to the west of Pinchincha in the Cordilleras of the Andes. These specimens were analysed in 1805 by Messrs. Fourcroy and Vauquelin, who found them to be different from the tabasheers of Asia. Instead of being wholly composed of silex, they contained only 70 per cent. of this earth, and 30 per cent. of potash, lime and water. The tabasheer which Dr. Brewster employed in his experiments was sent from Nagpore by Dr. Moore to Dr. Alexander Kennedy, who favoured Dr. B. with a considerable portion of it. It had the same general chemical characters as the tabasheer from Hyderabad, which was used by Mr. Smithson, the same specific gravity nearly, and the same external appearance ; whence Dr. B. had no hesitation in considering it as also composed of silex. When the semi-transparent specimens of this substance are immersed in water, they imbibe it with great rapidity, emitting numerous bubbles of air. The transparency increases whenever the air has been discharged, and after a few minutes the water pervades and renders transparent the whole mass. If a small portion of water, on the contrary, is laid upon the tabasheer when dry, instead of adding to its transparency as might have been expected, it renders it as opaque and white as chalk ; and from the same cause, the tabasheer which has been saturated with water becomes opaque as the water evaporates, reaches its maximum degree of opacity, and recovers its semi-transparency when perfectly dry. The increase of transparency from the absorption of water is an effect easily explained, and is one with which mineralogists have been long familiar in the phenomena of hydrophanous opal ; but the production of opacity by the absorption of a smaller portion of the same fluid which produces transparency is a fact entirely new, and not easily explicable upon known

principles. Dr. B. having ascertained that the white opacity was not the result of any chemical change, had recourse to optical causes; and he found by forming one of the semi-transparent specimens into a prism, that the production of the opacity was owing to this singular fact, that the refractive power of tabasheer is not only lower than water, but so much lower as to be almost intermediate between water and the gases. The physical properties of tabasheer are not less singular than its optical qualities, and indicate a structure of a very remarkable kind.

May 13.—A paper was read On the different Qualities of the Albumen of Spring- and Winter-felled Oak Trees, by T. A. Knight, Esq.

May 20.—Read An Account of certain Experiments made with a View of determining the Law of Attraction between Iron and a Magnetic or Compass Needle, by P. Barlow, Esq.

ROYAL INSTITUTE OF FRANCE.

Public Sitting of the four Academies.

In the public sitting of the four Academies of the Royal Institute of France, Mr. Charles Dupin delivered a discourse, the subject of which was the influence of the sciences upon the humanity of nations. In showing how far the sciences had not only softened the manners of mankind, but also the otherwise inexorable laws of war, Mr. Dupin quoted instances with respect to England and France, which claim the admiration of all the friends of civilization.

The following are the examples alluded to :

“ For three centuries we have witnessed the Learned Societies of all polished nations united in one fraternal bond; not only the learned of a single empire, but the most celebrated philosophers of all nations. From every quarter an appeal has been made to every talent, and prizes offered for the research of great truths, or their application to the useful purposes of mankind.

“ Crowns of merit have been awarded by the Amphictyons of science to the superior talent of all, without the invidious distinction of *native* and *foreigner*.

“ Nor has war restrained the limits of this peaceful concourse. The Society where Newton once presided, had founded a prize for the greatest discovery relative to the laws of light and heat. The theory of Malus respecting the polarization of light merited the prize. The judges were English, the author a Frenchman: the war was at its height, and the two countries were exasperated by victory and defeat, by the songs of a Tyrtæus and the harangues of orators, by fallacious pamphlets and the hirelings of a policy without shame or remorse.

“ But

“ But Justice held the balance with one hand, and the prism of Newton with the other;—admitting of no delusion, she gives her award in silence, uninfluenced by passion.

“ England presents her with no work equal to that of the learned Malus, and Justice places the crown on the brow of an enemy scarred with wounds, the honourable marks of battle waged between the two nations under the walls of Cairo and Alexandria.

“ Science is not only just—impassible only when equity requires it; she in every other case succours mankind with her benevolent aid.

“ During thirty years of war and bloodshed—Civilization, the daughter of Science, has maintained her rights, and often applied them to the noblest purposes.

“ Thus the Institute of France and the Royal Society of London have rivalled each other in generous philanthropy. At their intercession, captives have been liberated whose learning might be useful to mankind; and, to their praise be it spoken, the Governments on both sides the sea have always yielded with zeal to the solicitations of those scientific Institutions, who in gratitude have paid the ransom of the liberated by their presents.

“ The Academy of Sciences, by awarding to the celebrated Davy, about the same period, the prize for his Galvanic researches, showed itself equally impartial, and superior to the prejudices of popular hatred.”

XV. *Intelligence and Miscellaneous Articles.*

PYROLIGNEOUS ACID.

Extract of a Letter from M. G. C. at Paris, to Professor VAN MONS.

“ A DISCOVERY of the greatest importance engages at this moment the attention of the scientific world. A M. Monge has discovered that the pyroligneous acid obtained from the distillation of wood has the property of preventing the decomposition and putrefaction of animal substances. It is sufficient to plunge meat for a few moments into this acid, even slightly empyreumatic, to preserve it as long as you please. Cutlets, kidneys, liver, rabbits, which were thus prepared as far back as the month of July last, are now as fresh as if they had been just procured from the market. I have seen carcasses washed three weeks ago with pyroligneous acid, in which there is as yet no sign of decomposition. Putrefaction not only stops, but it even retrogrades. Jakes exhaling infection cease to do so as soon as you pour upon them the pyroligneous acid. You may judge how many important ap-

plications may be made of this process. Navigation, medicine, unwholesome manufactories, will derive incalculable advantages from it. This explains why meat merely dried in a stove does not keep, while that which is smoked becomes unalterable. We have here an explanation of the theory of hams, of the beef of Hamburgh, of smoked tongues, sausages, red herrings, of wood smoked to preserve it from worms, &c. &c."

Dr. Jorg, Professor at Leipsic, has since made many successful experiments of the same nature. He has entirely recovered several anatomical preparations from incipient corruption, by pouring this acid over them. With the oil which is produced from wood by distillation in the dry manner, he has moistened pieces of flesh already advanced in decay; and, notwithstanding the heat of the weather, soon made them as dry and firm as flesh can be rendered by being smoked in the smoking-room. All traces of corruption vanish at once when the *vinegar of wood*, or the *oil of wood*, is applied to the meat with a brush. The Professor has also begun to prepare mummies of animals, and has no doubt of success. He promises great advantages to anatomy, domestic œconomy, and even to medicine, from this discovery (for the remedy seems very fit to be applied internally and externally in many disorders), and intends to publish the result of his further experiments.

OXYGENATED WATER.

We noticed in our last this new liquid, obtained by M. Thénard in prosecuting his experiments on the oxygenized acids. It is a combination of oxygen with water. The quantity of oxygen contained by water when thus saturated is 850 times its volume, or twice its proper quantity. Its specific gravity is 1.453, and on being poured into water it sinks through it at first like a syrup, though it is very soluble. Applied to the skin, it has an instant action, and renders it white, causing a smarting, which continues longer or shorter as the quantity is more or less: if considerable, it destroys the skin. Applied to the tongue, it has the same effect, and thickens the saliva. Its taste cannot well be described, but it resembles that of an emetic. It acts powerfully on oxide of silver: each drop let fall on it dry, causes an explosion, with an evolution of light if in a dark place. The peroxides of manganese and of cobalt, and the oxides of lead, platinum, palladium, gold, iridium, and some other metals, act powerfully on oxygenated water. Many metals when minutely divided produce the same phenomena, as silver, platinum, gold, osmium, iridium, rhodium, and palladium. In all these cases the extra oxygen is disengaged, and sometimes that of the oxide; but in others a part of the oxygen combines with the metal, as with arsenic, molybdenum, tungsten, and selenium. These metals are acidified, frequently

frequently with the production of light. Acids render the oxygenated water more permanent. Gold, finely divided, acts with extreme force on pure oxygenated water, but has no action on that containing a little sulphuric acid.

MR. WILLIAM SMITH'S WORKS ON FOSSIL SHELLS.

To Mr. Tillock.

Plastow, July 18, 1819.

SIR,—Having been induced from what I had repeatedly read in your very useful Magazine, concerning Mr. William Smith's ingenious and practical investigations of the British strata, and in commendation of his intended works on *Fossil Shells*, to become a subscriber to two of them, which were a long time ago begun to be published in numbers; I shall offer no apology for requesting permission to address Mr. Smith, through your pages, in order to complain of the slow progress, and apparently unreasonable delay, which seems to attend the proceedings with regard to these works. The first of these, "Strata identified by organized Fossils," was begun to be published in June 1816, and was promised to be continued monthly, until it was completed in seven numbers; yet it was only during the last month that the fourth number thereof made its appearance. When does Mr. Smith intend to fulfil his engagements, with regard to the completion of this work?

The second of these works, "Stratigraphical System of organized Fossils," was proposed to be completed by Mr. Smith in two parts; the first of which was published in 1817, and yet the second and concluding part has not yet been published. When is this to take place?

I make these remarks, and put these questions, from no prejudice or ill will towards Mr. Smith: on the contrary, I highly value his services in the cause of practical and useful geology, and will, on the completion of these two works within a reasonable time, do all I can to extend the sale of them amongst my acquaintances.

I am, &c.

A SUBSCRIBER.

P. S. Myself and some of my friends made application at the British Museum, wishing to examine the geological collection of fossil shells, corals, &c. which Mr. Smith had deposited there, and had in part described in his Strat. Syst., but were told it was not in order, and could not be seen. Will Mr. Smith have the goodness, if he can, or some one else, to inform the scientific public, through your pages, whether this impediment to the spread of a practically useful species of knowledge has since been removed, and is the Smithian collection now accessible to the British public?

LITHIA.

Mr. Berzelius has given the following method of proving whether this new alkali be present in any mineral: A small fragment of the mineral, of the size of a pin's head, or a little of its powder, with a small excess of soda, is to be heated by a blow-pipe, for two minutes, on a piece of platinum foil. The mineral is decomposed, the lithia liberated by the soda; and the whole being kept fluid, at this temperature, by the excess of alkali, it spreads over the foil, surrounding the decomposed mineral. The part of the platinum near to the fused alkali becomes of a dark colour, more or less intense, and over a greater or less part of the surface, proportioned to the quantity of lithia in the mineral. This oxidation of the platinum takes place only around (not under) the alkali, where air as well as the lithia has access to the metal. Potash destroys the reaction of the platinum on the lithia, if the latter be not abundant. The platinum being washed and heated, resumes its metallic surface.

ALLOYS OF PLATINUM.

It results from some experiments by Mr. Fox of Falmouth, that if about equal bulks of platinum and tin, in contact with each other, be heated to redness, they combine suddenly with great vehemence, and a considerable extrication of light and heat, which continues for some time after they are removed from the fire. This experiment may be performed by enveloping a piece of tin in platinum foil, and exposing it on a piece of charcoal to the flame of a blow-pipe: a kind of explosion takes place at the moment of combination, and the alloy runs about like ignited antimony.

Similar effects take place with platinum and antimony. This alloy, when highly heated for some time, becomes solid and very malleable, and then contains but little antimony.

Zinc produces the same phenomena, exploding and burning at the moment of combination.

DREADFUL EXPLOSION IN A COAL-MINE.

It is our painful duty to notice another of those distressing accidents which so frequently occur in coal-mines. On Tuesday the 20th July, owing, it is reported, to some neglect or mismanagement regarding the safety-lamps in the Sheriff-hill pit, at Gateshead near Newcastle-on-Tyne, a dreadful explosion took place, by which nearly 40 persons have lost their lives. Had the accident happened an hour later, it is said about 100 persons would have perished.

BLASTING OF ROCKS.

Colonel Warnaghen, of the Brazils, has made an important discovery; he has ascertained that the sawdust of wood, especially of woods of the less harder sort, triples the force of the powder employed in blowing up rocks when mixed with it in equal parts.

VELOCITY OF SOUND.

It appears from experiments lately performed at San Jago, in Chili, by M. D. Josef de Epinosa and D. Felipe Bauza, that sound moves with a velocity of 1,227 English feet in a second, the air being at a temperature of 73.5 Fahr. and the barometer 27.44 inches.

FRENCH CHRONOMETER.

This chronometer consists of a needle perforated towards the middle by an axis fixed in the centre of a vertical dial-plate, and which turning freely around that axis has the property of indicating the hour upon the dial-plate.

It is not directed by any external moving power, and contains in itself all the principle of its action.

It completes in twelve hours its revolution around a dial-plate, whose divisions, not equal but proportional, are passed over in equal times, as those of a solar dial are by the shade.

It has no other point of support than the immovable axis round which it turns; its extremities are insulated, and have no communication with the centre.

If the needle is directed towards another hour than that which it indicates on the dial-plate, it returns of itself, like the needle of a compass when attempted to be diverted from its polarity, and after an extremely free oscillation recovers its first position; so that it can never stop or rest in apparent repose, but at that point of the circumference which corresponds with the time of day.

If the needle is separated from its axis and removed from the dial-plate, although to a situation where it is kept immovable, it does not lose any of its anterior properties; that is to say, on being replaced on the axis, it does not stop at the hour at which it pointed when last removed from the dial-plate, but seeks out and rests at the time of day when it is replaced.

It preserves for fifteen days all these properties; and they are renewed by a very simple operation.

It may be varied in its form and its dimensions. The specimen which is seen at Peschot's, the watchmaker, No. 18, Rue des Filles St. Thomas, one of the inventors, indicates the hour upon a dial-plate of thirty inches in diameter; it is in crystal, and the point which directs itself upon the hour is a gilded fleur de lis.

The axis around which it turns, may be fixed without injury upon a glass: the hours are then indicated upon that glass.

* * A scientific friend who has been lately in Paris, and has seen the above instrument, and its singular action, was completely at a loss how to account for it. Neither electricity nor magnetism, it is asserted, has any thing to do with its movements; so that its action must be entirely mechanical.

GEOMETRICAL RECREATION.

M. Allizeau, an artist of Paris, has invented a new sort of toy, to which he gives the name of *Metamorphoses*. It consists of five pieces, which may be so arranged as to compose all sorts of polygons; to resolve twenty-eight problems of geometry; and even to demonstrate the square of the hypotenuse. With the assistance of these pieces, a person may also trace plans of fortification; erect monuments, rustic villas, bridges, tombs, &c. The figures obtained are all regular and symmetrical; they result from a perfect square, susceptible itself of being divided into thirty-six other small squares. This invention may prove a source of very agreeable recreation, and cannot fail to be of great service to young people designed for the mechanical arts. It exhibits the first principles of architecture and geometry, a knowledge of which is indispensable in the practice of the arts.

This collection of geometrical recreations is composed, 1st, of five pieces, in wood, in ivory, or in mother of pearl, for the planes:—2d, of a small box inclosing the same number of pieces for the elevations;—3d, of a number of plates on which are designed fifty-six regular figures obtained from the composition of the square;—4th, of a second number of geometrical figures, &c. —5th, of a third number containing thirty-seven subjects of architecture in perspective elevation.—The price is fourteen francs (11s. 8d.)

PARTRIDGES.

At the latter end of last month, as a cat belonging to Mr. Allwork, of Goudhurst, was prowling through the meadows, it was observed to kill a partridge, and on examining the spot a nest was found, containing eighteen eggs, which were taken up and that evening deposited in an oven that had been recently used. On the following morning, when the oven was opened, the whole of the eggs were found hatched, and the young ones running about, but in catching them three were unfortunately killed; the remaining fifteen were put into the nest, and placed in the meadow from which it had been taken on the preceding evening. In a short time the old cock partridge was attracted to the spot, and in a few minutes it departed with the whole brood in the presence of several persons. Since that time they have been frequently seen by the gamekeeper of T. Wallis, esq.

METEOR.

A very remarkable meteor was seen at Aberdeen on Wednesday the 5th of May at about half past twelve in the forenoon. It appeared at an altitude of nearly 36 degrees, having the form of a ball of fire with a short tail darting towards the earth. The atmosphere was uncommonly clear at the time, with bright sunshine, and not a cloud to be seen. In about five minutes after it was observed, it exploded with a considerable noise, and a volume of smoke issued from it which assumed the form of a small white cloud. The same meteor was seen in many parts of the country. In the parishes of Kintora, Fintray, &c. the noise of the explosion was so loud that the cattle in the fields became terrified and bellowed loudly. It is very rare for such meteors to be visible in the day-time.—*The Scotsman*.

NEW COMET.

A new comet has within this month made its appearance in the constellation of the Lynx. It was seen in the northern parts of Britain and the continent before it was observed at London. At Edinburgh, York, Leeds, and Berlin, it was noticed on the 1st of July; at Hamburgh and Lauenburg on the 2d. It appears to have presented at Edinburgh a more luminous and distinct aspect than any where else to the southward. Its nucleus is represented as having appeared there very brilliant, and of about three-fourths of the diameter of Jupiter; and the whole breadth of the coma, or head, as about thrice the diameter of the nucleus.

At Berlin on the 1st, at 55 minutes past 11, it was in the meridian apparent altitude 2 deg. 20. min.; it was very brilliant, and visible in the next morning twilight 6 or 7 degrees above the horizon, appearing like Venus.

On the 3d, it was seen at the Observatory at Gosport, the learned superintendant of which (Dr. Burney) has furnished the public with the following observations upon it.

“ *To the Editor of the Star.*

“ Observatory, Gosport, July 4.

“ In the evening of the 3d of July 1819, from a quarter past nine till a quarter past twelve, we were gratified with the sight of a comet, with a lucid train projecting upwards or from the sun, and nearly in a perpendicular direction. At half-past ten, it was in the N. by W. point, within 10 degrees of the horizon, immediately in the breast of the Lynx, and by the sextant $19\frac{1}{2}$ degrees distant from Capella. At 10 h. 40 min. it was 44 deg. from Polaris, and at half-past eleven about 40 deg. from Dubhe in the back of Ursa Major, when it was due north, and had a slow motion downwards of about $2\frac{1}{2}$ degrees per hour. Viewed through a good achromatic telescope, its body appeared more
confused,

confused, or had a greater nebulosity, than when seen with the naked eye, perhaps from thick dewy haze then descending. Though the brilliancy of moonlight was not favourable to observations, yet the nucleus of the comet appeared of a pale white light, and was sometimes brighter than at others, as was also the tail, which expanded upwards at intervals from 6 deg. to 10 deg. in length by the sextant. From its position and motion it would appear that it had passed through the head of the Lynx, between Auriga and Ursa Major, and was now advancing towards the head of Gemini. The train had a little inclination westward, and appeared about 3 deg. in width at its greater extremity. Its apparent magnitude is nearly similar to the comet that appeared here at the beginning of September 1811; but the train is much longer and wider."

"Observatory, Gosport, July 8.

"MR. EDITOR—I beg leave to send you a few observations respecting the new comet, by way of *addenda et corrigenda* of the article that was hastily drawn up last Saturday night, and inserted in your paper of Monday, and am yours truly,

"WILLIAM BURNEY.

"The comet's north polar distance when passing the meridian about 12 P.M. on the 3d instant, was 44 degrees; and its north declination 46 degrees. On Monday night, when on the meridian, its north polar distance was 43 degrees, and its north declination 47 degrees nearly: so that its north polar distance decreases, and its declination increases. On Tuesday evening it could not be seen here, from the interposition of clouds. Last evening (the 7th) at 20 minutes past nine o'clock, mean time, it was 26 degrees west of due north; and 16 degrees above the horizon. Since Saturday night it has risen about 2 or $2\frac{1}{2}$ degrees towards the polar star, and receded from Capella nearly two degrees westward; which now makes its place on a good celestial globe about two degrees under the three small stars behind the left shoulder of the constellation Lynx. Now, since the sun is in the focus of a comet's parabolic or elliptical orbit, it is evident this comet, in its approximation to Polaris, is advancing to its perihelion, and from its slow motion, and the direction of its path, most likely we shall see it for a considerable time. It is about 25 degrees from the sun, and within two degrees of our zenith at noon; and, when most brilliant, its head is globular. Sometimes it appears as small as a star of the second or third magnitude, at other times equal to Saturn in apparent diameter, but of a lighter colour than that planet. The breadth of the head, including diffused *coma*, is nearly half the moon's apparent diameter. The tail is well connected with the head, without any perceptible aperture; but has not appeared on any evening to be so long by several

several degrees as on Saturday night, when it measured from six to ten degrees in length, and upwards of two degrees in breadth at its extremity."

The comet was observed at London and its vicinity, at Paris, at Brussels, &c. on the same evening as at Gosport, namely, on the 3d of July.

The following is a table of the observations made upon it at the Royal Observatory, Greenwich. On the 3d, 11th and 13th it was observed on the meridian below the pole; the observation of the 7th was made with the large equatorial in the eastern dome.

1819.	☉ Longitude.	Mean Time of Obser- vation.	Comet's A.R. in Time.	Declina- tion North.	Longitude.	Latitude North.
	S. ° ' "	H. M. S.	H. ° ' "	° ' "	S. ° ' "	° ' "
July 3	3 11 7 44	12 6 55 3	6 51 35 6	43 41 13	3 9 56 3	20 39 54
7	3 14 55 53	11 53 2 0	7 8 9 5	48 17 41	3 12 28 51	25 33 54
11	3 18 45 10	12 6 7 4	7 22 20 2	50 31 22	3 14 40 43	28 6 6
13	3 20 39 33	12 4 29 3	7 28 34 5	51 7 31	3 15 40 15	28 51 30

It was at first supposed that this comet was the same with that discovered some months ago by M. Pons; but from a notice on that comet which has been recently read to the Academy of Mar-silles, by M. Blanpain, Director of the Royal Observatory there, it appears that this is not the case. The comet discovered by M. Pons was not in the constellation Lynx, but that of *Leo*; and although it has ever since the 12th of June, the day of its discovery, regularly increased in apparent size and brilliancy, it is still very small, and quite invisible to the naked eye. Its nucleus is besides indistinctly marked; its rays are very faint; and it has no appearance of a tail. All these are circumstances which separate it distinctly from the comet which is now the subject of observation.

M. Bouvard, Member of the French Academy of Sciences and Astronomer of the Bureau of Longitude, has made the following calculation of the elements of this new comet.

Passage to the perihelion, 2d August, at twelve, mid-day.

Perihelial distance 0.51744

The distance of the earth from the sun being taken as unity.

Longitude of the perihelion 0° 47'

Longitude of the node 277° 14'

Inclination of the orbit 44° 57'

Heliocentric movement, direct.

M. B. however remarks, that these elements are as yet but approximations, and that this comet differs from all those which have been hitherto observed.

DEATH OF PROFESSOR PLAYFAIR.

Prof. Playfair, who has been for some time past in a declining state of health, died at his house in Forth-street, Edinburgh, on the 20th of July. His death is universally regretted. No man ever perhaps deserved or enjoyed a larger share of the public esteem. By the world at large he was respected for his great and various acquirements, both in literature and science, while to the circle of his private friends he was in a peculiar manner endeared by his mild and unassuming character.

LIST OF PATENTS FOR NEW INVENTIONS.

To Stephen Bedford, of Birchall-street in Birmingham, for his improvement in the preparation of iron and other metals for various purposes; and also an improvement in the converting British iron into steel.—22d June, 1819.

To David Gordon of Edinburgh, and Edward Heard of Brighton, for their invention of a portable gas lamp.—19th June.

To Alexander Hadden of Aberdeen, manufacturer, for an improved manufacture for carpeting.—22d June.

To Edward Jordan of Norwich, for his improved water wheel for draining marsh lands, whereby water may be raised from a greater depth by a wheel of less diameter, and a large quantity of marsh land drained in a shorter time than by any water wheel now in use, and thereby great labour and expense saved.—22d June.

To Edmund William Williams, of St. Mildred's-court, Poultry, for certain improvements in the mode or art of distilling, communicated to him by a person residing abroad.—26th June.

To William Brunton, of Birmingham, for certain improvements in steam-engines and furnaces of steam-engines, by which a saving in the consumption of fuel is effected, and the combustion of smoke is more completely attained.—29th June.

To Nicholas Conne, of St. Mary-le-Strand, Middlesex, glass engraver, for an improvement applicable to lamps for domestic purposes, communicated to him by a person residing abroad.—30th June.

To John Scheffer, of Church-street, Blackfriars-road, Surrey, for his machine or instrument for writing, which he denominates the Pennographic or Writing Instrument.—8th July.

To William Good, of Bridport Harbour, Dorsetshire, for his improvement in the art of tanning hides and skins, and barking or colouring nets, sails, and other articles, by the application of certain materials hitherto unused for that purpose.—10th July.

To Joseph Clisela Daniell, of Frome, Somerset, for certain improvements in dressing woollen cloths; also in preparing and using wire cards as applicable to that purpose.—17th July.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1819.	Age of the Moon.	Thermo- meter.	Baro- meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
June 15	22	60°	29.83	Cloudy—heavy rain morning and
16	23	54°	30.03	Showery [evening.
17	24	65°	30.25	Fine
18	25	62.5	30.20	Cloudy
19	26	64.5	30.30	Fine
20	27	65°	30.32	Ditto
21	28	63.5	30.20	Cloudy
22	new	57.5	30.13	Showery
23	1	64°	30.06	Cloudy
24	2	62°	29.85	Ditto
25	3	67°	29.85	Rain
26	4	62°	29.60	Showery
27	5	66°		Ditto
28	6	60°	29.67	Ditto—rain and hail with thunder and lightning, morning and af- [ternoon.
29	7	65°	29.56	Fine
30	8	67°	29.40	Cloudy—rain A.M.
July 1	9	61°	29.54	Fine
2	10	64°	29.56	Showery
3	11	72.5	29.40	Fine
4	12	81°	29.37	Ditto
5	13	75°	29.50	Cloudy—rain in the evening.
6	14	67.5	29.60	Ditto—heavy rain at night.
7	full	72°	29.80	Fine
8	16	72°	29.66	Cloudy
9	17	65°	29.72	Fine
10	18	61.5	29.72	Ditto
11				
12				
13	19	63.	29.86	Cloudy
14	20	66.5	29.83	Ditto

METEOROLOGICAL TABLE,
 BY MR. CARY, OF THE STRAND,
 For July 1819.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dry- ness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Mornin.	Noon.	11 o'Clock Night.			
June 27	57	63	55	29.70	37	Stormy
28	56	59	54	.78	33	Thunder-storms
29	57	67	55	.94	49	Fair
30	56	68	50	.80	40	Showery
July 1	58	69	52	.89	49	Fair
2	57	59	54	.88	0	Rain
3	56	67	59	.90	46	Fair
4	60	81	68	.84	55	Fair
5	68	78	67	.95	48	Fair
6	60	67	62	30.03	0	Rain
7	63	68	60	.27	37	Cloudy
8	62	64	55	.08	0	Rain
9	57	70	57	.09	43	Fair
10	63	73	56	.15	56	Fair
11	57	69	64	.19	40	Cloudy
12	67	74	65	.21	54	Fair
13	64	68	54	.28	42	Cloudy
14	56	63	60	.23	40	Cloudy
15	57	66	55	.10	48	Fair
16	55	64	57	.10	57	Fair
17	62	73	67	.12	60	Fair
18	68	74	67	.03	62	Fair
19	64	74	66	29.67	65	Fair
20	68	73	54	.42	55	Fair—heavy fall of rain at night
21	54	57	55	.56	26	Cloudy
22	56	67	60	30.10	57	Fair
23	60	69	66	.25	67	Fair
24	67	77	66	.25	51	Fair
25	68	75	66	.15	75	Fair
26	67	75	65	.10	93	Fair

N.B. The Barometer's height is taken at one o'clock.

XVI. *On Cohesion.* By Mr. HENRY MOSELEY.*To Mr. Tilloch.*

SIR, — **T**HE power of cohesion is universally allowed to be one of the most interesting and important properties of matter. The obscurity of its laws, the singularity of its effects, and its immediate connexion with the necessities and comforts of every class of society, claim for it a distinguished rank among the researches of the philosopher. Nevertheless no phenomenon of nature has received so small a share of philosophical investigation. This neglect is perhaps principally to be ascribed to an idea (very generally received), that the nature of the attraction of cohesion (as existing only between insensibly minute particles of matter) precludes the possibility of experimental research. As it relates to an analytical mode of investigation, this argument certainly holds good; but it is no objection to a synthetical investigation.

Convinced that the known properties of matter present us with sufficient data for ascertaining the true law of cohesion, and that the ill success of a few, whose aim was rather to reconcile experiment with theories the favourite phantoms of their own imaginations, than to deduce such theories from facts, did not stamp impossibility on the undertaking; I have been induced to commence an investigation of the subject, the result of which I beg leave to submit to the public through the medium of your valuable Magazine. I remain, yours, &c.

Canonbury, July 12, 1819.

HENRY MOSELEY.

A connexion between the attraction of cohesion and the attraction of gravitation has long been suspected by philosophers. It still, however, remains to *prove* the real existence of such a connexion, and to show how far it extends. In the following remarks I shall attempt to demonstrate that these two attractions are different effects of the same power, following the same law under different conditions. So numerous and powerful are the objections which present themselves to this hypothesis on a partial consideration, that its *possibility* will, I am afraid, be at once denied; and any arguments in its favour be treated as attempts to establish an hypothesis which daily experience contradicts. In order therefore that the subsequent arguments may obtain an unprejudiced consideration, I shall endeavour to remove the principal objections to the fact generally—that cohesion and gravitation are effects of the same power; and, having thus shown the possibility of my hypothesis, proceed to demonstrate its reality.

The most powerful and evident objection to this hypothesis is, that the cohesion of a body is in many instances more powerful

than its gravity. Now attraction being known to exist in proportion to the quantity of matter, and the attraction of gravity being exerted between a comparatively small and an immensely large aggregate of matter, whilst that of cohesion only exists between small aggregates; it seemingly implies that a less attraction is more powerful than a greater. In answer to this it may be stated, that gravity being exerted at exceedingly great, whilst cohesion only exists at insensibly small distances, the energy of the one can be no fair criterion to judge of that of the other. In the subsequent part of this paper it will be shown that gravity is an aggregate of attractions which would be individually insensible; whilst cohesion is an aggregate of individually sensible attractions. Now of the two masses between which gravity is exerted, one (the earth) being infinitely greater than either of those between which cohesion exists, it follows that the aggregate of *insensible* attractions is infinitely greater than the aggregate of *sensible* attractions. But this by no means precludes the possibility of the latter being infinitely stronger than the former: besides, it must be recollected that the cohesion of one particle is not merely the effect of the attraction of another,—every particle being spherical, is in contact with eighteen others; every effort therefore to remove a single particle will be resisted in a greater or less degree by at least nine other particles.

Another objection that has been urged is, that the power of cohesion diminishes much faster than the square of the distance: some, indeed, have gone so far as to assert, that it diminishes as the cube of the distance. By what means these conclusions are obtained I know not; except it be by approaching the particles of bodies, and observing mechanically their attractions at different distances. The power of cohesion acting only between particles, and these particles being insensibly small, can but extend its influence to insensibly small distances; and to attempt to measure the sensible decrease of a power which at sensible distances is insensible, is attempting to divide mechanically an insensibly small space;—the result of such an attempt cannot certainly be urged as an argument.

It may again be objected, by those who believe in the existence of repulsion between the particles of matter, that by this hypothesis no such power is accounted for. The existence of repulsion is not, however, by any means demonstrated; and all the experiments adduced in favour of it (with the exception of those connected with electricity and magnetism) may be accounted for without having recourse to such a power.

The three principal objections which apply generally to this hypothesis having been answered, I shall consider it admitted, that the attraction of gravitation and the attraction of cohesion may be the same, and proceed to prove that they are so.

The particles of bodies being individually too minute to come directly under the observation of our senses, we can only arrive at a knowledge of their nature and properties through the medium of the nature and properties of their aggregates.

The general admission of the two following positions, drawn from the known properties of bodies and our ideas of the wisdom and goodness of the great Author of Nature, may claim for them the rank of axioms*. 1st, That all matter is ultimately composed of infinitely minute particles. 2dly, That these ultimate particles are perfectly solid, impenetrable, and consequently immutable bodies. To these may be added a third; viz. That the particles of which all bodies, whether simple or compound, are *directly* composed, are homogeneous. For with respect to simple bodies this cannot be for a moment doubted; and of compounds we can form no other idea than that they are either a mixture of particles, or that by a certain action the differing particles combine, and thus the whole are rendered similar. That the mere admixture of particles is insufficient to produce a true chemical compound is universally allowed; we must therefore conclude, that the particles of which all bodies are directly formed are homogeneous. Now a chemical compound is produced by presenting the particles of different bodies to one another; and the particles of a chemical compound being homogeneous, it follows that the particles of the component bodies must have undergone a change, by which their individuality has been destroyed. The particles of which bodies are *DIRECTLY* composed, must therefore be penetrable and changeable properties, which according to our axiom cannot belong to the ultimate particles of matter. Hence it may be fairly inferred, that the particles of which bodies are directly formed, differ essentially from the ultimate particles of matter; and that bodies must be composed of at least two kinds of particles, which may be called primary or direct, and ultimate particles: the primary particles, it has been shown, are homogeneous; the ultimate particles are similar or dissimilar, according as they compose simple or compound bodies. Whether there be more than two descriptions of particles or not, does not appear†:—the primary particles may be composed of other secondary ones, these of others, and so on *ad infinitum*, where perhaps are the ultimate particles of matter.

The attraction called gravity is an universal property of matter: it is found in similar bodies in proportion to their masses or quantities of particles. It may therefore be inferred that this attraction exists in the particles of bodies as well as in the bodies

* In support of these axioms may be adduced the opinions of Sir I. Newton, Baxter, &c.

† On this subject I shall communicate something in the next Number of this Magazine.

themselves (for every particle added increases the attraction of the whole), and that the attraction of the whole is an aggregate of the attractions of its particles. But the particles of bodies being infinitely minute, the spaces in which their individual attractions are sensible must be infinitely small, and at sensible distances these attractions must become insensible. The attraction of a mass therefore at a *sensible* distance is an aggregate of individually insensible attractions; but at an *insensible* distance it becomes an aggregate of sensible attractions;—a conclusion which is supported by actual experiment: for if two rough surfaces be presented to one another, the attraction is insensible, the distance of their particles being sensible (and the aggregate not sufficiently great to render the individual attractions sensible); but if the distance be rendered (by polishing) insensible, the attraction immediately becomes sensible. The particles of bodies therefore, when placed at insensible distances, exercise sensible attractions in virtue of the attraction of gravitation, and at sensible distances this attraction (unless the masses be immensely large) ceases. The attraction of cohesion follows precisely the same laws:—may it not then be fairly inferred, that these attractions are one and the same?

If the primary particles of bodies be allowed to possess the power of attraction, it may be inferred on similar grounds, that the particles of which they are composed are likewise endued with it; we must therefore allow them cohesion, and that their cohesion and attraction is more powerful than that of the primary particles themselves: we must likewise conclude the form of a primary particle to be spherical.

It being admitted that the attraction of cohesion and the attraction of gravity are the same, it follows from a moment's consideration that the particles of different bodies must differ in size and in density.

Let a primary particle A containing a quantity of matter (a) be presented to another, B, containing a quantity of matter (b), and let the attraction at the surface of A be greater than that at the surface of B; *i. e.* let $\sqrt[3]{B} \times a$ be greater than $\sqrt[3]{A} \times b$, and let the contact of A and B be so minute that some of the secondary particles of D may be within the sphere of the attraction of those of A. Then, the attraction of the particles of A being greater than the attraction of the particles of B, it will also be greater than their cohesion; and their cohesion being destroyed, the attraction of the whole mass A being greater than that of B, these secondary particles of B will leave it, and form round A; and the operation will be continued till the two are formed into one, whilst every secondary particle exerting a similar attraction,

the

the operation may be carried on *ad infinitum* till the ultimate particles of each are placed together.

Let it be granted :

- 1st. That gravity and cohesion are effects of the same power following the same law ; *i. e.* diminishing as the square of the distance increases.
- 2d. That the particles of bodies are of different densities and magnitudes, and consequently of different gravities.
- 3d. That if two primary particles of matter, the attraction at the surface of one of which is greater than the attraction at the surface of the other, be brought within the sphere of the attraction of each other's particles, the two particles will combine and form one compound spherical particle.
- 4th. That the gravity of the whole is equal to the sum of the gravities of its component particles.

Let $a : b$ as the gravities of two equal masses A and B.

$c : d$ as the cohesion of A : cohesion of B. Then

PROBLEM I. — The magnitudes of the particles of A are to those of D as $(b^3 c^3)$ to $(a^3 d^3)$.

Let $x : y$ as magnitudes of particles.

Then $\frac{1}{x} : \frac{1}{y} ::$ the numbers of particles, the masses A and B being equal $a \div \frac{1}{x} : b \div \frac{1}{y} :: ax : by ::$ The gravities of the particles (Ax. 4th.); that is, as the quantities of matter they contain. $\sqrt[3]{x} : \sqrt[3]{y} ::$ diameter of x : diam. $y :: \frac{1}{x^{\frac{1}{3}}} : \frac{1}{y^{\frac{1}{3}}} ::$ squares of diameters inversely $\therefore \frac{ax}{x^{\frac{1}{3}}} : \frac{by}{y^{\frac{1}{3}}} :: a \sqrt[3]{x} : b \sqrt[3]{y} ::$ the attraction at the surface of x : the attraction at the surface of y and the number of particles exerting this attraction on any one particle being under all circumstances the same ; viz. 9 :: $c : d$ $\therefore \sqrt[3]{x} : \sqrt[3]{y} :: bc : ad$, and as $x : y :: b^3 c^3 : a^3 d^3$. Q. E. D.

Corollary 1st.—The numbers of particles contained A and B are as $\frac{1}{b^3 c^3} : \frac{1}{a^3 d^3}$.

Corollary 2d.—The gravities of the particles of equal masses, or the gravities of wholes containing equal numbers of particles, are as $a \div \frac{1}{b^3 c^3} : b \div \frac{1}{b^3 c^3} :: c^3 b^2 : d^3 a^2$.

Corollary 3d.—The diameters of particles as $bc : ad$.

Corollary 4th.—The cohesions are as the specific gravities of wholes multiplied into the diameters of the particles.

PROBLEM II.—If any number of particles of the mass A be presented to an equal number of particles of the mass B, so that the particles of A and B may attract one another, and form other

compound particles. Or in other words, equal numbers of particles of A and B be formed into a chemical compound $A + B$, then shall the specific gravity of $A + B$ be to the specific gravity of

$$A \text{ as } \frac{1 + \left(\frac{d}{c}\right)^3 \times \left(\frac{a}{b}\right)^2}{1 + \left(\frac{ad}{cb}\right)^3} : 1.$$

By the last proposition :

$$b^3 c^3 : a^3 d^3 :: \text{magnitude particles of } A : \text{mag. part of } B.$$

By Corollary, $c^3 b^2 : d^2 a^2 ::$ gravities of particles.

By Axiom the third :

$$\text{As } b^3 c^3 + a^3 d^3 : b^3 c^3 :: \text{mag. particles } A + B : \text{mag. part. } A.$$

$$\text{As } c^3 b^2 + d^3 a^2 : c^3 b^2 :: \text{gravities of part. } A + B : \text{grav. part. } A.$$

As $\frac{1}{b^3 c^3 + a^3 d^3} : \frac{1}{b^3 c^3} ::$ numbers of particles of $A + B$, and A contained equal masses ;

$$\therefore \frac{c^3 b^2 + d^3 a^2}{b^3 c^3 + a^3 d^3} : \frac{1}{b} :: \text{specific gravity of } A + B : \text{spec. grav. } A.$$

$$:: \frac{c^3 b^3 + d^3 a^2 b}{b^3 c^3 + a^3 d^3} : 1 :: \frac{1 + \frac{d^3 a^2}{c^3 b^2}}{1 + \frac{a^3 d^3}{c^3 b^3}} : 1 :: \frac{1 + \left(\frac{d}{c}\right)^3 \times \left(\frac{a}{b}\right)^2}{1 + \left(\frac{ad}{cb}\right)^3} : 1. \text{ Q. E. D.}$$

PROBLEM III.—The conditions being the same as in the last problem. The cohesion of $A + B$: the cohesion of $A ::$

$$\frac{1 + \left(\frac{d}{c}\right)^3 \times \left(\frac{a}{b}\right)^2}{1 + \left(\frac{ad}{cb}\right)^3} : 1.$$

$$\text{By the 4th corollary } \sqrt[3]{b^3 c^3 + a^3 d^3} \times \frac{1 + \frac{d^3 a^2}{c^3 b^2}}{1 + \frac{a^3 d^3}{c^3 b^3}} : \sqrt[3]{c^3 b^3} ::$$

$$\sqrt[3]{1 + \frac{a^3 d^3}{b^3 c^3}} \times \frac{1 + \frac{d^3 a^2}{c^3 b^2}}{1 + \frac{a^3 d^3}{c^3 b^3}} : 1 :: \frac{1 + \left(\frac{d}{c}\right)^3 \times \left(\frac{a}{b}\right)^2}{\left(1 + \frac{ad}{cb}\right)^3} : 1. \text{ Q. E. D.}$$

PROBLEM IV.—If a chemical compound be formed with any two bodies A and B in the proportion of $g : 1$ (g being the number of particles of A added to every one of B , and not exceeding $\frac{d^3 a^2}{c^3 b^2}$), then shall the specific gravity of $A + B$ be to the specific

$$\text{gravity of } B \text{ as } \frac{1 + \frac{g c^3 b^2}{d^3 a^2}}{1 + \frac{g c^3 b^3}{d^3 a^3}} : 1.$$

Then

Then Corollary 2d, $c^3 b^2 : d^3 a^2 ::$ gravities of masses containing equal numbers of particles or gravities of particles contained $\therefore g c^3 b^2 : \text{as sum of the gravities of the particles of A added to every particle of B to form the compound particles of A+B}$
 $\therefore g c^3 b^2 + d^3 a^2 : d^3 a^2 ::$ gravities of particles of A+B : grav. of particles of B; and (Prob. 2d), as $b^3 c^3 + d^3 a^3 : d^3 a^3 ::$ mag. of the particles $\therefore g b^3 c^3 + d^3 a^3 : d^3 a^3 ::$ mag. part. A+B :

mag. part. B $\therefore$ (Prop. 3d), $\frac{g c^3 b^2 + d^3 a^2}{g c^3 b^3 + d^3 a^3} : \frac{1}{a} :: \frac{g c^3 b^2 a + d^3 a^3}{g c^3 b^3 + d^3 a^3} : 1 ::$

$$\frac{1 + \frac{g c^3 b^2}{d^3 a^2}}{1 + \frac{g c^3 b^3}{d^3 a^3}} : 1 :: \frac{1 + \left(g \times \frac{c}{a}\right)^3 \times \left(\frac{b}{d}\right)^2}{1 + \left(g \times \frac{c b}{d a}\right)^3} : 1. \quad \text{Q. E. D.}$$

PROBLEM V.—The conditions being the same as in the last problem, the cohesion of A + B will be to B as

$$\frac{1 + \left(g \times \frac{c}{d}\right)^3 \times \left(\frac{b}{a}\right)^2}{\left(1 + g \times \frac{c b}{a d}\right)^3} : 1$$

For Corollary 4th $\sqrt[3]{g h^3 c^3 + a^3 d^3} \times \frac{1 + \frac{g c^3 b^2}{d^3 a^2}}{1 + \frac{g c^3 b^3}{d^3 a^3}} : d a :: \text{co-}$

hesions :: $\frac{1 + \frac{g c^3 b^2}{d^3 a^2}}{1 + \frac{g b^3 c^3}{d^3 d^3}} : 1 :: \frac{1 + \left(g \times \frac{c}{a}\right)^3 \times \left(\frac{b}{d}\right)^2}{\left(1 + g \times \frac{c b}{a d}\right)^3} : 1. \quad \text{Q. E. D.}$

Examples.—Let it be required to find the ratios of the magnitudes and gravities of the component particles of lead and tin, and the weights of masses containing equal numbers of particles. The specific gravity of lead being 11.3525, its cohesion 29.25, and the spec. gravity of tin 7.2914, its cohesion 49.25.

Here $a : b :: 11.3523 : 7.2914$. Log. 29.25 = 1.46613

$c : d :: 29.25 : 49.25$. Log. 7.2914 = 0.86279

Log. $c b$ = 2.32892

Log. 11.3523 = 1.05500

Log. 49.25 = 1.69214

Log. $a d$ = 2.74714

Log. $c^3 b^3$ = 6.98676

Log. $a^3 b^3$ = 8.24142

As Log. $c^3 b^3 : \text{Log. } a^3 d^3 :: 6.98676 : 8.24142 :: \text{Log. } 1 : \text{Log. } 17.97 \therefore c^3 b^3 : a^3 d^3 :: 1 : 17.97 :: \text{magnitude particles of lead to magnitude particles of tin. (Prop. 1st.)}$

$$3 \times 1.69214 = 5.07642$$

$$3 \times 1.46613 = 4.39839$$

$$2 \times 1.05500 = 2.11000$$

$$2 \times 0.86279 = 1.72558$$

$$\text{Log. } d^3 a^2 \quad \overline{7.18642}$$

$$\text{Log. } c^3 b^2 \quad \overline{6.12397}$$

Therefore, as $\text{Log. } c^3 b^2 : \text{Log. } d^3 a^2 :: 6.12397 : 7.18642 ::$
 $\text{Log. } 1 : \text{Log. } 11.5464 \therefore c^3 b^2 : d^3 a^2 :: 1 : 11.5464 ::$ gravities
of particles of lead : gravities of particles of tin, or of masses
of lead to masses of tin containing equal numbers of particles.
(Cor. 2d.)

Example 2d.—Let it be required to find the specific gravity
and corpuscular attraction of a compound body formed by melting
together masses of lead and tin containing equal numbers of
particles.

$$\text{PROBLEM II.}—\frac{1 + \left(\frac{d}{c}\right)^3 \times \left(\frac{a}{b}\right)^2}{1 + \left(\frac{a d}{c b}\right)^3} : 1 :: \text{spec. grav. comp.} : \text{spec. grav. lead.}$$

By last example,

$$\left.\begin{aligned} \left(\frac{d}{c}\right)^3 \times \left(\frac{a}{b}\right)^2 &= 11.5464 \\ \left(\frac{a d}{c b}\right)^3 &= 17.97 \end{aligned}\right\} \therefore \frac{1 + \left(\frac{d}{c}\right)^3 \times \left(\frac{a}{b}\right)^2}{1 + \left(\frac{a d}{c b}\right)^3} = \frac{12.5464}{17.97} = .6615$$

$\therefore .6615 : 1 :: \text{spec. of comp.} : \text{spec. grav. lead} \therefore 11.3523 \times$
 $.6615 = 7.3354645 = \text{spec. gravity of composition.}$

$$\text{PROBLEM III.}—\text{As } \frac{1 + \frac{d^3 a^2}{c^3 b^2}}{1 + \left(\frac{a^2 d^3}{c^3 b^3}\right)^{\frac{2}{3}}} : 1 :: \text{cohesion of comp.} : \text{cohesion of lead.}$$

Here, as before, $\frac{d^3 a^2}{c^3 b^2} = 11.5464$. $1 + \frac{a^3 d^3}{c^3 b^3} = 18.97$. Log.
 $18.97 = 1.27807 \times \frac{2}{3} = .85204 = \text{Log. } 1 + \left(\frac{a^3 d^3}{c^3 b^3}\right)^{\frac{2}{3}} = \text{Log. } 7.113$

$$\therefore \frac{1 + \frac{d^3 a^2}{c^3 b^2}}{1 + \left(\frac{a^3 d^3}{c^3 b^3}\right)^{\frac{2}{3}}} = \frac{12.5454}{7.113} = 1.763 \therefore \text{cohesion of composition}$$

to cohesion of lead as : 1 : 1.763.

Example 3d.—Let it be required to find the specific gravity
and corpuscular attraction of a compound formed by melting to-
gether lead and tin in the proportion of 11.5 particles of the
former to 1 of the latter*.

$$\text{PROBLEM IV.}—\text{As } \frac{1 + \frac{11.5 c^3 b^2}{d^3 a^2}}{1 + \frac{11.5 c^3 b^3}{a^3 d^3}} : 1 :: \text{spec. grav. comp.} : \text{spec. grav. lead.}$$

* Or equal weights of the two metals. See Example 1.

$$\text{Log. } \frac{11.5 \, c^3 \, b^3}{d^3 \, a^3} = \text{Log. } 11.5 + \text{Log. } c^3 \, b^3 - \text{Log. } b^3 \, a^3 = 1.06070 \\ + 6.98676 - 8.4142 = -.19396 = \text{Log. } .6397.$$

$$\text{Log. } \frac{11.5 \, c^3 \, b^2}{a^2 \, d^3} = \text{Log. } 11.5 + \text{Log. } c^3 \, b^2 - \text{Log. } a^2 \, d^3 = 1.06070 \\ + 6.12397 - 7.18642 = -.00175 = \text{Log. } .996 \therefore \frac{1 + \frac{11.5 \, c^3 \, b^2}{d^3 \, a^2}}{1 + \frac{11.5 \, c^3 \, b^3}{a^3 \, d^3}} =$$

$$\frac{1.996}{1.390} = 1.436 : 1 :: \text{specific grav. of composition} : \text{spec. gravity} \\ \text{tin } 1.436 \times 7.2914 = 8.8736338 = \text{specific gravity of the com-}$$

$$\text{pound } \frac{1 + \frac{11.5 \, c^3 \, b^2}{a^2 \, d^3}}{1 + \frac{11.5 \, c^3 \, b^3}{a^3 \, d^3}} = \frac{1.996}{1.639713} = \frac{1.996}{1.390} = 1.436 : 1 :: \text{cohesion}$$

of tin to cohesion of compound.

The specific gravities found in the two last examples agree exactly with those deduced from actual experiment. The truth of the principle on which they are calculated may therefore be considered as demonstrated. The cohesions of bodies, particularly when small, are exceedingly difficult to ascertain. A plan which, however, I have lately adopted, will, I hope, enable me to obtain something satisfactory on this head; and I have no doubt but the result will amply confirm the truth of my hypothesis. In fact, the calculation of the specific gravity being founded on the very same principle as that of the cohesion, (viz. that cohesion is inversely as the square of the distance,) the truth of the one is a proof of that of the other.

From the formulæ given above may be calculated the specific gravity and cohesion of a chemical compound of any two substances, the specific gravities and cohesions of which are known; they may therefore be extensively applied in the practice of metallurgy as well as chemistry.

In a future number of the Philosophical Magazine I shall give various formulæ for calculating the specific gravities and cohesions of compounds formed by any given numbers and proportions of different bodies, and for finding the proportions in which any number of bodies must be combined, in order to produce a given specific gravity or cohesion; as well as various new methods of ascertaining the cohesions of bodies. By means of these formulæ and those already given, the portion of alloy contained in gold or silver may be found at once from their specific gravity; and the proportion in which any compound metal, as brass, &c. is compounded, from the specific gravity of such metal.

XVII. *Observations on the Relation of the Law of Definite Proportions in Chemical Combination, to the Constitution of the Acids, Alkalis, and Earths.* By JOHN MURRAY, M.D. Fellow of the Royal College of Physicians, of the Royal Society of Edinburgh, the Geological Society of London, &c., Lecturer on Chemistry, and Materia Medica and Pharmacy.*

THE law that every body enters into chemical combination in a certain equivalent weight to others, and that when it combines in different proportions with another, these proportions have a simple arithmetical ratio, is perhaps the most important that has hitherto been discovered in the science of chemistry. It is now so far established, notwithstanding some difficulties which attend it, that when a view of the constitution of an extensive series of chemical compounds is brought forward, different from what had hitherto been proposed, it is incumbent to show that it is consistent with the operation of this law; and if just, this may display relations not before observed, and may obviate objections which have arisen from a different view. It is from these considerations that I submit the following observations on the application of the law of definite proportions to the theory which I have proposed of the chemical constitution of the acids, alkalis, and their compounds. It necessarily leads to considerable modifications of these applications; and the conclusions which these afford, if I am not deceived, afford proofs of the truth of the opinion I have advanced, and lay open some new views. The subject is at the same time so extensive, as to have relations to nearly all the details of chemistry.

In the preceding paper† I remarked, that the relations in the proportions of oxygen and hydrogen forming the supposed portion of combined water in the acids, will probably be those of one or both of these elements directly to the radical. It remained to be determined how far this is just.

Sulphur affords the best example for illustration, as its combinations with oxygen and hydrogen are capable of being accurately determined.

Sulphur and oxygen are held to combine in two definite proportions, forming sulphurous and sulphuric acids. In the first, 100 parts of sulphur are combined with 100 of oxygen; in the second, 100 are combined with 150 of oxygen, forming what is called the real acid, with which are further combined 56·7 of combined water, the entire compound, constituting the acid in the highest state of concentration, (1·85 of specific gravity) in which it can be procured in an insulated form.

This constitution of these compounds appears at first view in

* Communicated by the Author.

† See Phil. Mag. vol. 52. p. 107 and 195.

opposition to the law of definite proportions in chemical combinations; for, according to that law, the higher proportion of an element in combination with another is a simple multiple of the lower proportion in which it combines with the same body. And hence, since in the first combination of sulphur with oxygen, 100 of the former are combined with 100 of the latter; in the second, 100 ought to be combined with 200, while the combination is that of 100 to 150. And in the atomic hypothesis, this involves the absurdity of supposing, that while, in the first compound, the combination, in conformity to the common rule, is that of one atom of sulphur with one atom of oxygen; in the second, it is that of an atom of sulphur with an atom and a half of oxygen. To obviate this, it is supposed that a combination of sulphur with a lower proportion of oxygen exists,—an oxide composed of 100 of sulphur with 50 of oxygen. The ratio will then be that of 1, 2, 3 of oxygen in the three compounds to one of sulphur. And in the atomic system, the first will be held to be that of an atom of sulphur with an atom of oxygen; the second, that of an atom with two atoms; and the third, that of one with three. To this, however, it may be objected, that no such oxide of sulphur can be obtained, though if it were a possible combination, it ought, from the law of attraction, that the first proportion of an element is retained in union with the greatest force, to be the one most permanent and most easily obtained.

Whether this objection be just or not, the difficulty can be solved without any hypothesis, on the view that the elements of the supposed water exist in the composition of the acid; for the quantity of oxygen in this water is just 50; of course, the entire quantity is the regular proportion 200. And the composition is 100 of sulphur, 200 of oxygen, and 6·7 of hydrogen.

This result favours the conclusion, that the relation of the oxygen in common sulphuric acid is entirely that of this element to sulphur; that it is therefore in immediate combination with the radical; and hence, that water does not exist in the constitution of the acid. And even if the existence of an oxide of sulphur and of what is called real sulphuric acid were admitted, the combinations would be strictly conformable to the law of proportions, being those of one of sulphur to 1, 2, 3, and 4 of oxygen.

The proportion of hydrogen is also determined by its relation to the sulphur; for its quantity is that in which they combine, 6·7 of hydrogen with 100 of sulphur constituting the composition of sulphuretted hydrogen.

It thus appears that the proportions of both elements are determined by their relation to the sulphur as the radical of the acid, and are those which the quantity of sulphur would separately require. This, so far as theory can discover, is not a necessary.

sary result. The oxygen and hydrogen might each have required the quantity of sulphur with which they combine,—that is, the existing relations might have been those of sulphur to oxygen, and sulphur to hydrogen, in their several proportions. It is otherwise; there is the relation of sulphur to oxygen, and in addition to this of hydrogen to the same sulphur. And thus, since the same quantity of sulphur receives the acidifying influence of both elements, we discover the source of the higher degree of acid power. How water should augment acidity, no principle enables us to conjecture. But how the joint operation of two elements acting on the same quantity of radical which each of them separately is capable of rendering acid, should augment the effect, is easily perceived. And even from this consideration alone, there can remain little hesitation in admitting the conclusion, that both these elements act directly on the sulphur,—in other words, that the three are in simultaneous combination.

As there is no proof of the existence of oxide of sulphur, and as no such compound as that denominated real sulphuric acid, composed of 100 of sulphur with 150 of oxygen, can be obtained insulated, it might be supposed that the hypothesis of such combinations ought to be excluded; and that the strict fact only should be admitted, of the two compounds which constitute sulphurous and sulphuric acids.

There is one ground, however, on which it may be inferred that a relation of sulphur to oxygen, in the proportion of 100 to 150, exists. When sulphuric acid is acted on by a base neutralizing it, its hydrogen combines with a portion of its oxygen forming water. The quantity of oxygen thus abstracted is 50, and, of course, the above proportion remains; and this being admitted, the existence of oxide of sulphur, it may be supposed, must also be assumed to bring the results under the law of definite proportions. And the combinations of oxygen to sulphur will still be in the ratio of 1, 2, 3, 4.

This conclusion, however, does not follow; for in cases where this apparent result happens, the oxygen which is abstracted forming water is replaced by the oxygen of the base, and makes up the proportion of 200 to 100 of sulphur; and the new compound is a ternary combination of these elements in these proportions with the metallic radical of the base. A single example will illustrate this. 30·7 of common sulphuric acid require for saturation 69·6 of oxide of lead; the former is composed of 10 of sulphur with 20 of oxygen, and 0·7 of hydrogen, the latter of 64·6 of lead with 5 of oxygen. The hydrogen in their mutual action abstracts 5 of oxygen forming water, and there remain 20 of oxygen, 10 of sulphur, and 64·6 of lead in combination. The same result is established in all other cases, and they afford

no evidence, therefore, of the existence of any such compound as that of real sulphuric acid.

But there is another case which does not admit of the same explanation, and in which the relation of 1 of sulphur to $1\frac{1}{2}$ of oxygen seems to be demonstrated. It is that of the action of sulphurous acid on salifiable bases. Here, as there is no abstraction of oxygen in the formation of water, while there is the addition of the oxygen of the base, the proportion in the combination is that of $1\frac{1}{2}$ to 1 of sulphur. This will be apparent from the same example of oxide of lead. 20 of sulphurous acid composed of 10 of sulphur and 10 of oxygen combine with 69.6 of oxide of lead, composed of 64.6 of lead and 5 of oxygen: supposing a simultaneous combination to be established, the proportions will be 10 of sulphur, 15 of oxygen, and 64.6 of lead; and supposing the two latter to observe a relation to sulphur, the proportion is that of 100 to 150 of oxygen.

It might be maintained that no change of composition in the two binary compounds, the sulphurous acid and oxide of lead, takes place, but that they merely unite; or, at least, that while the sulphur and lead display their peculiar relation to each other, each of them retains its relation to oxygen. But this is inconsistent with the general view which has been given of the state of a neutral compound, and can scarcely be supposed to exist with regard to one case, when the reverse is maintained with regard to others.

At the same time, the relation of 100 of sulphur to 200 of oxygen is fully established in common sulphuric acid. Whether it is necessary to admit that of 100 to 50, except on the atomic hypothesis, is not apparent, but it is not improbable.

The same view may be applied to the illustration of the acids of which carbon is the radical. I have remarked in the preceding paper, that the vegetable acids are to be regarded, not according to the doctrine of Lavoisier, as composed of a compound radical of carbon and hydrogen acidified by oxygen, but as compounds of a simple base, carbon, acidified by oxygen and hydrogen. On this principle the question occurs, What is their precise composition? The proportions assigned by the analyses hitherto given appear at variance with every principle, and can be brought under no law, nor any analogy whatever; nor has this been attempted. Part of this may arise from the difficulties of the analysis, but more of it perhaps is to be ascribed to the composition not having been considered under the just point of view;—in more recent investigations, particularly in which only accurate experimental results can be expected, to the idea having been entertained that they contain a portion of combined water in their insulated state, which they yield when combined with a base, and that

that the composition of the acid is to be determined, abstracted from this water, and as it exists in combinations in which it is supposed to be in what is called its real state. The principle which I have applied to their constitution leads to very different results.

In conformity to the law, which it has been shown exists with regard to sulphur, it is probable the oxygen and hydrogen will be in the definite proportions which they separately observe to carbon. And from the different proportions in which they combine with this element, a number of compounds may be thus formed.

Carbon, with the first proportion of oxygen, forms an oxide. Hydrogen is an acidifying power. Its addition, therefore, it is not improbable, may give rise to acidity, and its proportion will be determined either by its first or second proportion to carbon, or by both. Carbon, with its second proportion of oxygen, forms a weak acid. The addition of hydrogen to this will no doubt augment acidity, and its proportion will also be determined by its first or second proportion to carbon, or both. Four specific compounds will thus be established, which will be represented by carbonic oxide with a certain proportion of hydrogen; one, that which exists in carburetted hydrogen; the other, that in supercarburetted hydrogen; and by carbonic acid with similar proportions of hydrogen. Further, there has appeared reason to infer the existence of a relation in proportion of sulphur to oxygen intermediate between that of sulphurous and sulphuric acid; a similar relation may exist in the case of carbon, intermediate between carbonic oxide and carbonic acid; and with the addition of hydrogen, may give rise to acidity. Lastly, there is some reason also to suppose the existence of a combination of sulphur with oxygen in a lower proportion than that in sulphurous acid. There may be a similar combination with carbon, which may also, with an additional proportion of hydrogen, produce acidity. It remains to inquire how far the composition of any of the vegetable acids can be brought under these laws.

Carbonic acid is the binary compound of carbon and oxygen. With the addition of hydrogen there is every reason to infer, that, as in the case of all the other binary acids containing oxygen, an acid will be formed of increased power. Oxalic acid is the strongest of the vegetable acids; and the results of its analysis will be found to lead to the conclusion that it is this ternary compound.

Berzelius submitted oxalic acid to experiment by combining it with oxide of lead, drying the oxalate and decomposing it by heat. His object in following this method was to abstract the combined water of the acid, and to operate upon it in what is considered as its real state. He accordingly found, that the acid

loses

loses water in entering into this combination; and he objects to a preceding analysis by Gay-Lussac, in which the oxalic acid had been operated on in the state of oxalate of lime, as in this combination the water of composition is not abstracted. His objection is valid, on the doctrine which has been universally adopted by chemists, of acids containing water essential to their constitution, which is abstracted when they enter into combination with a base, such as oxide of lead, in which water is not retained. And if oxalic acid in passing into this combination lose water, as is the case, then on this idea its constitution ought to be determined from its analysis as it exists in a dry oxalate, exactly as that of sulphuric acid is inferred from its analysis in the state in which it exists in a dry sulphate. The reasoning of Berzelius, therefore, was relatively just; and on these data his results, though they have been objected to, as they involve difficulties in the atomic hypothesis, are correct. But in conformity to the doctrine I have illustrated, it is evident that the composition of the acid is not thus obtained, and that what exists in a dry oxalate, such as oxalate of lead, is a different combination. The crystallized oxalic acid is what ought to be submitted to analysis if it contained no water of crystallization; but as it does contain a portion, this is to be removed, without abstracting what has been called water essential to the acid. It exists in this state in oxalate of lime; and hence the results given by Gay-Lussac (if experimentally correct, and they appear to be singularly so) give its real composition. They are accordingly strictly conformable to the view I have stated of the composition of this acid. The proportions he assigns are 26.56 of carbon, 70.69 of oxygen, and 2.75 of hydrogen*. Now carbonic acid is composed of 27.4 of carbon, and 72.6 of oxygen. The proportion of carbon and oxygen, therefore, in oxalic acid, is precisely the same; and the sole difference in composition from carbonic acid is in the proportion of hydrogen it contains.

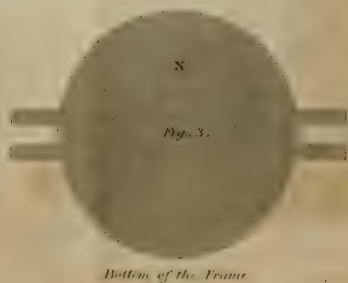
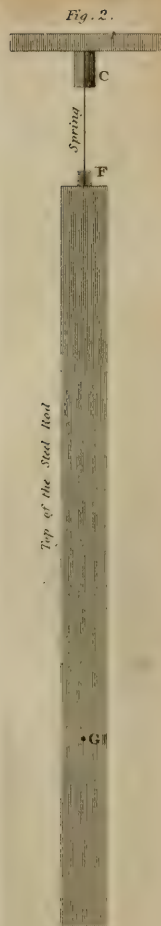
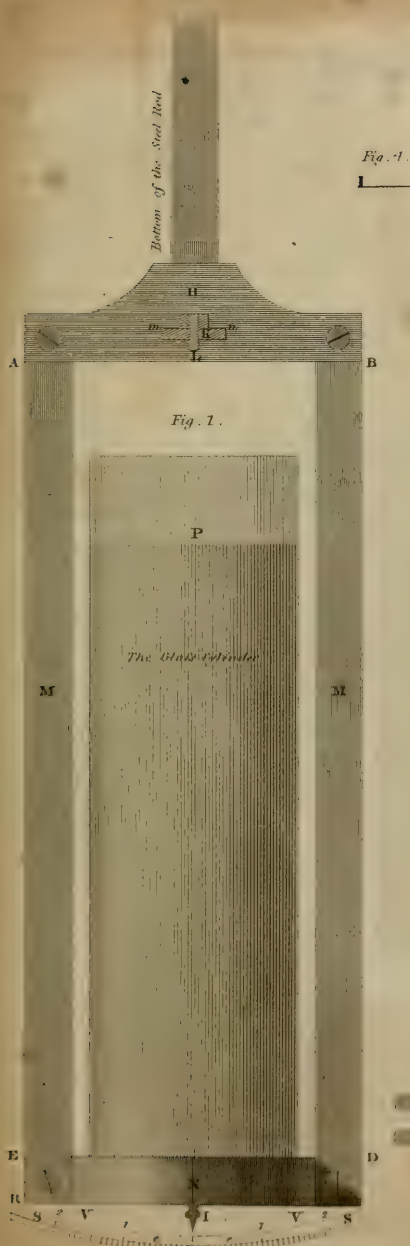
The constitution of oxalic acid may likewise be inferred indirectly from the method of Berzelius: and it will be satisfactory if a coincidence is thus obtained. The composition of the real acid, as it is called, existing in oxalate of lead, is stated by Berzelius at 33.22 of carbon, 66.53 of oxygen, and 0.25 of hydrogen. But to this, to express the true composition of the acid, are to be added the proportions of oxygen and hydrogen expended in the formation of water, in the mutual action of the acid and the oxide of lead. The quantity of hydrogen is inferred from the quantity of oxygen: and there are different principles connected

* *Recherches Physico-Chimiques*, tome ii. p. 302.

with the doctrine, as has been already illustrated in considering the action of sulphuric acid on a base, whence the proportion of oxygen may be determined. Thus, it must be a multiple of that existing in the composition of what is called the real acid, or in the composition of the known definite compounds of carbon and oxygen, or it is equivalent to the oxygen in the base, this quantity of oxygen being always abstracted in the mutual action in combination with the requisite proportion of hydrogen. Adopting this last principle as the most direct, 106 parts of real oxalic acid, it appears from Berzelius's analysis, combine with 307.5 of oxide of lead: this quantity of oxide contains 22.06 of oxygen, which is therefore to be added to the composition of the acid, with the proportion of hydrogen equivalent to this, 2.94. Hence this quantity of acid, 125 parts, is composed of carbon 33.22, oxygen 88.59, hydrogen 3.19: or in 100 parts the acid consists of 26.5 of carbon, 71 of oxygen, and 2.5 of hydrogen, proportions almost the same as those assigned by Gay-Lussac, and affording a coincidence on a difficult subject of experimental investigation that does honour to the accuracy of these chemists.

There can thus remain no doubt, that the proportion of carbon to oxygen in oxalic acid is the same as that in carbonic acid. The sole difference between them is in the proportion of hydrogen which the former contains; the one is the binary, the other the corresponding ternary compound, similar to what exists in other acids; and hence also, in conformity to the analogy of these acids, and to the principle which accounts for their acidity, is explained the difference in their acid powers.

The compound existing in a dry oxalate, such as oxalate of lead, ought to contain no hydrogen; for the whole of this element, like the hydrogen of sulphuric acid, must, in the action of the base, be combined with oxygen, and abstracted in the state of water. The small portion of hydrogen, therefore, stated by Berzelius, must be considered as derived from error of experiment; and its presence would be admitted more readily from the idea of some portion of hydrogen being essential to the constitution of the acid, as necessary to form what was regarded as its compound radical. In subsequent experiments, accordingly, Berzelius found reason to infer that the proportion was smaller than he had at first assigned. The minute quantity which he does suppose to exist in real oxalic acid, (less than 1 per cent.) he brings forward as a difficulty in the atomic hypothesis. A fraction of an atom, he remarked, cannot be supposed; and therefore the small quantity of hydrogen must be considered as an entire atom. But from the proportions it must be held to be combined with 27 atoms of carbon, and 18 atoms of oxygen, that is, with 45 other atoms,



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—a combination certainly altogether improbable; and any arrangement that can be conceived scarcely lessens the difficulty. Mr. Dalton endeavoured to obviate this, by supposing, that in the analysis of Berzelius the hydrogen is under-rated. But the reverse is the case. The solution may now be easily given. In the composition which properly constitutes oxalic acid, the proportion of hydrogen is sufficiently large to present no difficulty. And in what was considered as real oxalic acid existing in the dry oxalates, there is no reason to suppose that hydrogen exists. It is also obvious, that the proportion of oxygen and carbon in a dry oxalate is that constituting carbonic acid; for although in the action of the acid on the base a portion of its oxygen is abstracted with its hydrogen, a corresponding portion of oxygen is added from the base or metallic oxide, and a ternary compound is established.

The proportion of hydrogen indicated in the composition of oxalic acid is not conformable to either of the two proportions of carbon and hydrogen, which constitute the two compounds at present admitted as constituting the only definite compounds of these elements, the carburetted and supercarburetted hydrogen. It is much less even than that in the latter, which contains the lower proportion. Yet there is every reason to conclude, from the law which has been illustrated in reviewing the composition of sulphuric acid, that it must exist in a definite relation to the simple radical of the acid, that is to the carbon, conformable to the other relations which subsist between them. It follows, therefore, either that there is an error of analysis, in consequence of which the proportion of hydrogen is greatly underrated, or that there are other definite proportions in which carbon and hydrogen combine, than those which are at present admitted. The coincidence in the results of the analysis by Gay-Lussac and by Berzelius, in a great measure precludes the former supposition; and indeed an error so great would require to be assumed, as cannot be supposed. The other conclusion therefore follows: it is rendered more probable by other considerations, which give force to the opinion that hydrogen and carbon enter into more numerous proportions than have been assigned: And it is nearly established by the results of this case itself. Supercarburetted hydrogen is composed of 100 of carbon with 17.5 of hydrogen; carburetted hydrogen of 100 with 35. In oxalic acid, 26.5 of carbon are combined, according to the analysis of Berzelius, with 2.5 of hydrogen, which is in the proportion of 100 to 9.4. Now this deviates little, and not more than what may fairly be referred to inaccuracy in the estimation of the proportions in one or other of the compounds, from the fourth of the highest proportion

tion, that in carburetted hydrogen* ; and hence, in conformity to the law usually observed, hydrogen probably combines with carbon in proportions following the ratio of 1, 2, 3, 4 ; and taking a mean which further investigation may determine with precision, 100 of carbon may be supposed to combine with 9, 18, 27, and 36 of hydrogen. The proportion in oxalic acid will be conformable to the first of these relations, or half that in supercarburetted hydrogen.

Tartaric acid, which is next in strength to the oxalic, or is even equal or superior to it in acidity, appears to be the same combination with a larger proportion of hydrogen.

Gay-Lussac employed tartrate of lime as the medium to decompose the acid. In this state, while the water of crystallization of the acid is excluded, its composition is not subverted, for there is in the formation of tartrate of lime no abstraction of what is called combined water. The results therefore give the real constitution of the acid. The proportions he assigned are carbon 24.05, oxygen 69.3, hydrogen 6.62. Berzelius operated on tartrate of lead. The proportions he assigns are carbon 35.98, oxygen 60.21, hydrogen 3.807. But in the formation of tartrate of lead by the action of the oxide on the acid, a large quantity of water is formed. This being taken into calculation, his results agree perfectly with those of Gay-Lussac.

The proportion of the carbon to the oxygen, it is evident, is not much different from that which constitutes carbonic acid ; and the deviation is not greater than may fairly come within the allowance due to errors liable to be present in a subject of analysis so difficult.

The proportion of hydrogen is much larger than that in oxalic acid : it must, however, in conformity to the law which has been stated as regulating the proportions in ternary acids, bear a certain relation to the radical of the acid, that is to the carbon. And it is interesting to discover that this larger quantity is precisely the other definite proportion which it has appeared from these illustrations must be inferred to exist in the combinations of carbon and hydrogen. The two known proportions, those existing in supercarburetted and carburetted hydrogen, are 100 of carbon to 18 of hydrogen, and 100 to 36 ; the other two are those of 9 and 27. The first was found in oxalic acid, and the other is discovered in tartaric acid, the proportion in the above analysis of 24.05 to 6.62, being that of 100 to 26.5.

In the remaining vegetable acids the composition is evidently less perfectly determined, partly from the difficulty of procuring

* The composition of either of the carburetted hydrogen gases is not so well determined, as to exclude a correction sufficient to establish a perfect coincidence.

them insulated, and partly from the sources of error which attend the experiment, and which have not been checked or detected by the application of a just principle. It is therefore only from repeated experimental investigation, aided by such an application, that precision can be expected to be obtained. Still some of these results afford very high approximations to the views I have illustrated.

The proportions I assign are those founded on the analyses by Berzelius, corrected by the theory I have stated. He combined the acid with oxide of lead, and submitted it to decomposition in this state; the water of composition he supposed to be thus abstracted, and the real acid obtained. But the composition of the acid is in fact subverted, and the water is formed from the combination of its hydrogen with a portion of its oxygen. The quantity of oxygen thus lost is discovered by the quantity of oxide which the acid saturates, being equal, according to the principle already explained, to the quantity of oxygen in the oxide. The hydrogen lost is the quantity equivalent to this. And these quantities of oxygen and hydrogen being added to the proportions assigned by Berzelius, give the real composition. It is further necessary to remark, that as there has appeared reason to infer the existence of 4 definite proportions of oxygen with sulphur, observing the ratio of 1, 2, 3, 4, and 4 proportions of hydrogen with carbon in the same ratio, so there will be found equal reason to infer the existence of 4 similar proportions of oxygen with carbon, 100 of carbon being combined in the first with 62.5 of oxygen, in the second with 125 constituting carbonic oxide, in the third with 187.5, and in the fourth with 250 constituting carbonic acid. With these preliminary observations it is sufficient to give the general results*.

Citric acid appears to be carbon with oxygen in the third definite proportion, that between carbonic oxide and carbonic acid; and its hydrogen is nearly in the first proportion of that element to carbon.

Acetic acid is carbon with oxygen in the second proportion nearly, and with hydrogen in exactly the second proportion, that of 100 to 18. It is represented therefore by carbonic oxide, with hydrogen in the proportion which constitutes supercarburetted hydrogen.

Gallic acid is carbon with oxygen in none of the four definite proportions, but almost exactly in the mean proportion between the first and second. Its hydrogen is nearly in the first proportion of that element to carbon.

Succinic acid is carbon with oxygen in the second proportion,

* Under the history of the vegetable acids in the 4th volume of *System of Chemistry*, the precise proportions will be found.

that constituting carbonic oxide. The hydrogen conforms to none of the four proportions, but is the precise mean between the first and second.

In saccho-lactic acid the relation of the oxygen to the carbon is not that of any of the definite proportions, but is highest to the third. The hydrogen is that which constitutes supercarburetted hydrogen.

The analysis of benzoic acid is evidently very doubtful, owing to the difficulties which attend it from its volatility. It is the only one in which the proportion of oxygen to carbon is less even than the lowest of the definite proportions of these elements. The proportion of hydrogen is almost exactly that of the first proportion.

If the definite proportions of oxygen and hydrogen to carbon be assumed to be more numerous than 4, but still observing the law of simple multiples, all these results may be easily brought under the law. The relations suggested by these researches, and particularly those which prove that proportions of carbon both to oxygen and to hydrogen exist inferior to the lowest known proportions of these elements, afford much support to the conclusion, that their definite combinations are more numerous than the few that have been admitted, either on the doctrine of equivalents, or on the atomic hypothesis. And on the latter, the composition of organic compounds may be accounted for with this conclusion, so as to preserve what constitutes its chief excellence,—the principle that one body in a combination is always in the relation of one atom, and which is confessedly incapable of being maintained, with the assumption merely of the few definite proportions of the elements that have hitherto been assigned.

The view indeed that the vegetable acids are compounds of a simple radical (carbon) acidified by oxygen and hydrogen, and the law existing in this and other ternary combinations, that two of the elements observe the requisite relations in proportion to the third as a base, may probably be extended to all the vegetable, and perhaps even to the more complicated animal products; and, with the admission of a more extensive series of definite proportions in the primary elements, may remove the necessity of the law advanced by Berzelius, and apparently now admitted by the supporters of the atomic system,—that while in inorganic bodies one of the constituents is always in the state of a single atom, in organic bodies it is not so, but very often the reverse. If this law be excluded, and the reverse established, it will assimilate the constitution of organic to that of inorganic compounds, and must contribute greatly, independent of uniformity and simplicity, to render that of the former, at present so involved in obscurity and discordance, more precise.

[To be continued.]

XVIII. *On the Lunar Atmosphere.* By THOMAS FIRMINER,
LL.D.

IT is said in the Number of the Philosophical Magazine for June, (vol. 53. p. 465) that Mr. J. B. Emmett observed an occultation of a small star by the moon on the fifth of December 1818; and that he saw the star when “really behind the moon’s disc.” It would, I think, afford some satisfaction to many of your astronomical readers to know how this gentleman ascertained that the small star spoken of, was actually behind the moon’s disc at the time he saw it; he could not I think be assured of it by observation only, and as to calculation, the place of the moon is not so accurately computed as to assure him that the star was actually behind the moon when he saw it. The slow motion of the sun, in rising or setting, and the great refractive power of our atmosphere, enable us to know, from calculation, that he is below the horizon, when visible above it; but these circumstances do not take place at the moon. I have observed in occultations of stars at the moon’s bright limb, that their light diminishes as they approach towards the moon, and in a few seconds before the occultation they appear very small, and seem to vanish gradually. A very remarkable instance of this circumstance I observed whilst I was assistant to Dr. Maskelyne, and which is recorded in the account of the Greenwich Observations: but I always considered this appearance to have arisen from the superior brightness of the moon to that of the star, when very near its enlightened limb; the apparent magnitude of the star being rendered almost a point at the instant of its disappearance; but when the star emerges at the moon’s dark limb, it emerges with almost its full splendour. The appearance is also the same when the star immerses at the dark limb behind the moon. Whether the star has immersed or emerged at the moon’s dark limb, the appearance has been always instantaneous in all the occultations that I have seen. In all the eclipses of the sun yet recorded, the circular section of the sun, formed by the moon’s limb, is always regular and well defined; which I think would not be the case, had the moon an atmosphere sufficiently dense to occasion a refraction so great as Mr. Emmett observed. Whether the occultation he has recorded took place at the moon’s enlightened or dark limb does not appear in the journal; but I think, from the collective testimony of my own observations, that Mr. Emmett was led to conclude, that the star was behind the moon’s limb, from some deceptive appearance. In the instance I have alluded to, the star did not appear to vanish instantaneously, but gradually to disappear behind the moon’s enlightened limb. I however never supposed this appearance to have been caused by re-

fraction, it being so totally different when the occultation took place at the moon's dark limb. If the moon has an atmosphere, its existence is not, I think, likely to be discovered by such observations; as the rays of light coming from a star to an observer on the earth's surface, will not suffer any perceptible change in its direction from passing through it, although its density should be as great as at the earth's surface. The only difference in the appearance will be in the quantity of light, which difference is perceptible; but whether it arises wholly from the star being near to the moon, or partly from the light of the moon and partly from a lunar atmosphere, remains yet to be decided; and perhaps one of the best means to decide this doubtful point, would be to make accurate observations on the approach and recession of stars towards and from the moon's dark and enlightened limb in occultations: but for this purpose a much greater number should be computed than is usually inserted in the Nautical Almanac.

XIX. *On the Compensation Mercurial Pendulum of Mr. GAVIN LOWE. Communicated by THOMAS FIRMINGER, LL.D.*

THE late Mr. Gavin Lowe of Paradise-row, Islington, a gentleman eminently skilled in the mathematics and in the knowledge of theoretical and practical astronomy, is known to have made some years since a considerable improvement in the quicksilver pendulum, by which it may easily be adjusted for the effect of temperature, without removing it from the clock to which it is attached,—an advantage which Graham's and the other forms of compensation pendulums do not possess. Mr. Lowe, having taken great pains to compute the dimensions of the several parts of his pendulum from the best tables of expansion of steel, glass, and quicksilver, the materials of which the pendulum is composed, and also the quantity of quicksilver necessary to keep the centre of oscillation stationary in all the variations of temperature, applied it to his own transit clock; and finding the pendulum to perform to his expectation, he recommended it to a few time-keeper makers of his acquaintance, who made several pendulums from his directions. As the construction is simple, and the pendulum easily made by any ingenious workman, and as trials of its performance have shown that the clocks to which it has been applied, have gone as well with it as with any other kind of compensation pendulum of more complicated and expensive construction; and likewise as no improvement appears to have been made upon it since the death of the inventor, it seems to be the only kind of quicksilver pendulum now in general use. After having simplified his pendulum as much perhaps as it will admit, Mr. Lowe wrote

wrote out directions for its construction and for its adjustment, which he communicated to me in the year 1804, together with the investigation by which he had computed the invariable distance of the centre of oscillation: and as I believe these directions have been communicated to only a few of his acquaintance, and being informed by some time-keeper makers that they will be of great service—from these reasons, and in testimony of the gratitude I feel in the memory of my late learned friend, I have communicated the whole, both directions and investigation, to the public.

THOMAS FIRMINGER.

Description of a Mercurial Pendulum constructed by
Mr. GAVIN LOWE.

“ Fig. 2 (Pl. II) shows the top of the pendulum rod, made of steel $\frac{1}{8}$ of an inch thick, $\frac{3}{8}$ of an inch wide, and rounded at G to receive the fork of the crutch, so that there may be as little slack or play as can be perceived. Fig. 3 is the bottom, seen vertically, of the frame that holds the glass cylinder (P. fig. 2.), which is filled with quicksilver $6\frac{1}{10}$ inches from the bottom of the glass inside up to P. CF is the spring at top, one inch long and *pretty stiff*. G is the point where the crutch should embrace the pendulum rod. The crutch should be a fork of steel the same as the pendulum rod is. From C to G should be about 7 inches. L is the bottom of the steel rod, and $\frac{2}{10}$ of an inch of it to be turned into a screw that has 40 threads or turnings to an inch. The whole length of the rod, from the rivet that joins the spring to its top at F, to the end of the screw at L, to be $33\frac{9}{10}$ inches. The side pieces of the frame MM to be of steel as thick as the pendulum rod, that is $\frac{1}{8}$ of an inch, but not less than $\frac{1}{10}$. The top of the frame H consists of two pieces of steel each $\frac{1}{8}$ or $\frac{1}{10}$ of an inch thick, shaped as in the drawing, and screwed over the ends of the side bars MM. The inside height of the frame from E to A to be $8\frac{2}{10}$ inches; and the inside width between the bars MM, about $2\frac{1}{2}$ inches, so that the glass may stand $\frac{1}{8}$ of an inch clear of them. The bottom piece N is of cast brass about $\frac{1}{2}$ an inch thick from E to R, and formed as in the figure (3), with a base which may be hollowed down $\frac{1}{10}$ of an inch, so as to fit the bottom of the glass. The nut K is $\frac{1}{4}$ of an inch deep, and the diameter of its circle from m to n is $1\frac{6}{10}$ inch, having the upper edge divided into 25 equal parts, and figured 0, 1, 2, 3, or at each 7th division; each of those parts is very nearly equal to 1'' of time in 24 hours. A small bit of brass, shown in profile in fig. 4, to be screwed on below H, to point to the divisions on the nut K. A brass lid is made to fit the mouth of the glass *freely* or without *pinching*; the edge of the lid has two notches $\frac{1}{8}$ of an inch

deep, that receive the edges of the side bars M M ; a piece of white writing paper should be gummed on upon the bottom part of the lid. I is the index at bottom, and SS a scale of inches divided into tenths, and about 44 inches from C, the top of the pendulum. Try on this scale the arc of escapement; and if it should be $1\frac{2}{10}$ inch from *e* to *e*, then apply such a pulley weight that the pendulum shall vibrate from V to V, or $3\frac{6}{10}$ inches. The diameter of the glass being 2 inches inside very nearly, and the depth of the quicksilver $6\frac{4}{10}$ inches, the weight will be 10lbs. fully, and therefore it will be best to buy 11lbs.

“From accurate calculation I find, that if such a pendulum should go perfectly true when the thermometer is at 30° , but that at 90° it should go one second slower in 24 hours, it would be remedied by pouring in 10 ounces more quicksilver; or taking out that quantity, if it went one second faster in 24 hours at 90° , than 30° of the thermometer; and for $\frac{1}{10}$ of a second that the clock may go faster or slower in 24 hours, its compensation is obtained by putting in or taking out one ounce of quicksilver for those temperatures: and this is all easily done.

“Paradise Row, Islington, June 1804.

GAVIN LOWE.”

“Let the glass be wiped out with a large old clean linen rag before the quicksilver is poured into it. A paper funnel folded so that the end of it will reach the bottom of the glass is best for putting in the quicksilver. The extremity of its end should be closed by wrapping a bit of thread twice or thrice round, and knotting it. About 15 or 20 pricks should be made above the extremity of the funnel with a needle of the size that carries six-penny thread. During the filling in of quicksilver the person that holds the funnel should keep the pricks in the paper below the surface of the quicksilver that is in the glass, raising his hand as it fills up.”

(Copied August 24, 1804.)

T. FIRMINER.

On the Calculation and Construction of a Mercurial Pendulum for a Clock.

“The absolute quantity of expansion of the following metals was determined by the late Gen. Roy: viz.

“ 10 inches of plate brass with 60° of Fahrenheit	} Inches.	
expanded		0.0063
“ 10 inches of steel		0.0038
“ 10 inches of white flint glass		0.0027

“In 1792, an accurate experiment was made to find the difference between the expansion of white flint glass and quicksilver; for

for which purpose a thermometer of white flint glass with a large bulb and long stem was filled to the top when the temperature was precisely 32° ; it was then put into steam of 212° , and the quantity of quicksilver that ran over was weighed; the result was that 10000 grains gave $155\cdot48$ grs. that expanded or run over. This is the excess or difference between the expansion of the quicksilver and glass, and from which the following conclusion is drawn: That a cylinder of white flint glass filled at 32° of the thermometer with quicksilver to the height of 10000 inches, at 212° the column of quicksilver would be $10155\cdot48$ inches long.

"As $10000 : 155\cdot48 :: 10 \text{ inches} : 0\cdot15548$ for a difference of heat $= 180^{\circ}$; and therefore 10 inches for a difference of heat $= 60^{\circ}$, will give $0\cdot0519$ very nearly.

"From the above data the calculation and construction of a mercurial pendulum is easy. Let C B D represent such a one, having the rod and frame at bottom of steel, and a white-flint glass cylinder resting on the bottom of the frame, and filled with quicksilver to such a height, DB, that its expansion (which is upwards) shall be somewhat more than double the expansion of the whole rod from C to D. Suppose then that C D is 42 inches: then as 10 inches steel $:: 0\cdot0033 :: 42$ inches to $0\cdot01596$
2



The expansion of the quicksilver nearly $= 0\cdot03192$
But $0\cdot0519$ inch comes from 10 inches of quicksilver, therefore $0\cdot03192$ will come from $6\cdot2$ inches, which is nearly the height of the column; add $\frac{1}{27}$ of $6\cdot2$ inches to the column, or $0\cdot2 + 6\cdot2 = 6\cdot4$ inches, which is very nearly the length of the column. Let $CD = 42\cdot2$ inches, then $CD \div 6\cdot4 = 35\cdot8$ C B.

"Put $CB = a = 35\cdot8$, $BD = c$ $6\cdot4$. To find the centre of oscillation $\odot$, or its distance from C. The formula is $C \odot = \frac{3a^2 + 3ac + c^2}{3a + \frac{3c}{2}}$.

"Let us suppose then, that the pendulum, having C B and B D of the above dimensions, is constructed and set a-going at 30° of Fahrenheit: it is required to find,

"1st. The centre of oscillation.

"2d. To find it a second time when the thermometer is at 90° and C D and B D expand with 60° of heat.

" $3a^2 = 35\cdot8 \times 35\cdot8 \times 3 = 3844\cdot92$, $3ac = 35\cdot8 \times 6\cdot4 \times 3 = 687\cdot36$.

" $C^2 =$

" $C^2 = 6.4 \times 6.4 = 40.96$, $3a = 107.4 \frac{3c}{2} = 9.6$; therefore,

$$\frac{3a^2 + 3ac + c^2}{3a + \frac{3c}{2}} = 117)4573.24(39.08753 \text{ for the distance of centre of oscillation for } 30^\circ \text{ of the thermometer.}$$

" If 10 inches of steel with 60° expand 0.0038 , then 42.2 will expand 0.0160 ; therefore $42.2 + 0.0160 = 42.2160 = C D$. If 10 inches quicksilver expand 0.0519 , 6.4 gives 0.0332 , and $6.4 + 0.0332 = 6.4332 = c$. Hence $42.2160 - 6.4332 = 35.7828 = a$. And from these numbers the centre of oscillation is found as above $= 39.08782$ at 90° of thermometer; which differs from the former deduction, or when the thermometer stood at 32° , only 0.00029 .

" It is well known that when the ball of a pendulum is let down 0.01 of an inch, it will go 10 seconds slower in 24 hours; and therefore 0.001 will be equal one second, and 0.0001 equal $\frac{1}{10}$ of a second in 24 hours. It follows from this, that the above pendulum is so nearly corrected, that its daily rate at 90° of thermometer would be nearly $0''29$ slower than at 30° , and that the correcting column is rather too short; but the glass cistern itself expands the same way as the quicksilver, and the excess of its height above the top of the included mercury will fully compensate the error of $0''00029$. It likewise follows that the steel rod 42.2 inch, being lengthened 0.0160 , it would (if there were no compensation from the quicksilver) go $16''$ slower at 90° than at 30° of the thermometer. But since 6.4 inches of quicksilver compensates this, $\frac{4}{10}$ of an inch will compensate one second. For if the pendulum went one second slower in 24 hours at 90° than at 30° of the thermometer, the remedy would be to add $\frac{4}{10}$ of an inch more quicksilver. A glass cistern 2 inches diameter inside, and filled with quicksilver to 6.4 inches will contain 10 pounds weight very nearly, and therefore 6.4 inches give 10 lbs. or 160 ounces, $\frac{1}{10}$ of an inch gives $2\frac{1}{2}$ ozs. that is one ounce of quicksilver added or subtracted will very nearly compensate $0''1$ of the variation of the daily rate at 30° and 90° of the thermometer.

" There are five other things that ought to be particularly attended to in constructing a pendulum of this sort, which are not deduced from theory, but the results of 4 years experience.

" 1st. The spring at top should be one inch long, and of a proper stiffness.

" 2d. Let the crutch have a steel fork to embrace the steel rod at 7 inches from C, the point of suspension, and be free, with as little shake as possible.

" 3d. Let a scale of inches and tenths be placed at 44 inches from C, for the purpose of determining the arc of vibration and
also

also the arc of escapement. When the pendulum is truly in beat, measure on this scale the arc of escapement.

“ 4th. Apply a weight to the pulley that will make the arc of vibration from side to side three times the arc of escapement. This is easily done by common lead shot in a bag, which when of sufficient weight may be weighed, and the regular pulley weight constructed. If the arc of escapement from side were one inch, then the pulley weight ought to be so great that it may make the index at the bottom move over three inches from side, and so in proportion for any other arc of escapement.

“ 5th. Let the screw at the bottom of the rod have 40 turns or threads to the inch, and the outer edge of the circle of the nut be divided into 28 equal parts; when the nut is moved one of these divisions, it alters the going of the clock one second in 24 hours, very nearly.

“ With regard to the five things above mentioned, some people will say, Let the crutch be short; that is, about 4 or $4\frac{1}{2}$ inches long, but no more. This is a very vague direction; for it can be proved not only by calculation, but by experiment, that the shorter the crutch is, the more pulley-weight is required to give the pendulum the requisite arc of vibration; and this unfortunately loads the pivots of the barrel or cylinder, that the pulley-string goes round, occasioning a great deal of friction, and a consequent irregularity from foulness, or thickening of the oil in cold weather.”

XX. *Remarks on Madeira, Climate of the Tropics, Trade-Winds, Rio Janeiro, the Polar Ice, &c. Extracted from a Journal kept by JOHN HAMMET, Esq. in a Voyage from England to Rio Janeiro. Communicated by Dr. PEARSON.*

MADEIRA.

MR. VERTCH, who has been British consul at Madeira these many years, and is, besides, a wine-merchant of the first respectability, gave us the following opinion concerning the comparative virtues of Madeira and Teneriffe wines. It was simply, “ that bad Madeira is better than good Teneriffe.” Dr. Thornton, however, who has written on the medicinal virtues of good Teneriffe, differs in this respect, I am aware, widely with this accomplished and hospitable gentleman. Mr. V. also asserted that no Teneriffe wines are allowed to be imported to Madeira. Then, since what has been long said concerning the consumption in the British dominions of wines under the denomination of Port, may with equal propriety be expressed of the consumption of wines under the denomination of Madeira, the unjust substitution at home,

home, of Teneriffe for Madeira, can only take place with the British wine-merchants and tavern-keepers.

I learned from Dr. Henry, a resident and native of this island, that the disease implied by consumption is by no means uncommon among the natives, and that there are in it, at present, three families, Portuguese natives, in which a marked hereditary predisposition to this disorder prevails. From this gentleman I likewise learned a very uncommon source of consumption, that is but too common here. In consequence of the very high lands of Madeira, and the steep ascents to the multitude of houses above and about the town of Funchal, the labouring classes are occasionally under the necessity of bearing heavy burdens on their heads and necks, and in their extreme anxiety to obtain relief, are impelled to a temporary exertion, and corresponding quickness of motion, that are impulsive of an extraordinary flow of blood to the heart and lungs, while, at the same time, the peculiar action of the muscles above, from the duration of the forced inclination of the head and neck, necessarily impedes its ascent.

He further informed me, that in October, November, December, and January, the weather is warm down at Funchal; while, on the contrary, it is extremely cold up in the mountains. The merchants' families begin to go up into the mountains in spring, and to return down in the end of October or beginning of November.

The prevalence of elephantiasis, and its dreadful effects on the eyelids and eyes, I had an opportunity of witnessing in the lazaret-house to the left of Funchal. It seems principally to attack the poor; but some in good circumstances are likewise sorely afflicted with it. In all these cases, the digestive organs have been known, or at least said, to be out of order prior to the occurrence of the disease. One of the chief articles of food with the poor here is salted and dried fish, commonly without a due proportion of solid vegetable food as a corrective. The prickly heat induced by the continual and excessive perspiration from blood assimilated from such food, and, for the most part, aggravated by dirt, from inattention to indispensable ablutions, and thereby injuring in a greater degree the extreme vessels, *rete mucosum*, and exhalants, cannot but tend, with the cold atmospheric changes here, to bring on this disease.

During the 3d and 4th of November (the time allotted for our stay at this island) the temperature was between 73 and 70.

On the 2d and 3d the sky was really beautiful: at noon it might well vie with what the mind is led to conceive of an Italian sky; and in the evenings, the azure was every where streaked with the fleecy and crimson, and imbued with the sombre and golden hues, in a manner calculated to invite the affections, and
enrich

enrich the imagination. Such in general is the appearance of the tropical skies to the eye of the observing voyager, and such in general is their natural effect on his mind when cultivated in the sources of sensibility. Yet these, such as they are, fall short of the influencing beauties of the equatorial skies. Here, I am really of opinion, that persons who are accustomed to the sight only of the more northern* or southern skies, though they have unquestionably their peculiar, attractive, and enlivening beauties too, cannot possibly form a just conception of the exquisite beauty of the equatorial firmament, particularly where the calms, I am about to mention, prevail. The matter of light (or, as you long since divulged to the world, that modified state of calorific evinced by its producing the sensation denominated vision), reflected from the green surface of the deep, where no clouds, no vapours intervene, seems to extend far beyond the atmospheric realm, so remote does the ethereal concave seem, in consequence of the pure and peculiar pallid azure thus produced; and where they do, they appear, especially at noon, pendent, not in lowers and in mists, but in clusters, and in streaks of a perfect fleecy or snowy whiteness and pellucid clearness, in consequence of the pervasion of every particle of their attenuated bodies with the darting rays of a perpendicular sun; and in the east and west points of the horizon, on the rising and setting of this great agent of light and heat and life to our system, they assume, according to their particular densities, and relative positions with regard to one another, all the primary and various hues in variegated splendour. At night too, the moon, particularly when in opposition and “near her highest noon,” reflects her borrowed light through the attenuated clouds, as well as air, with peculiar lustre. Here, even with a common telescope her opacities can be distinctly observed; which with the starry firmament around are apt to lead the mind into an infinity of space, and matter, and perplexing circumstances, that completely bewilder it;—especially as the in-

* In no place north did I at any time *occasionally* notice such a beautiful clear sky, as while I was in the *Prevoyante*, lying in the open and spacious roads of Dantzic, in 1815 and 1817, although it was in the September and October months, when mists condense and clouds accumulate rapidly. I was told that in summer and in autumn it is particularly clear and delightful; a circumstance rather promoted than counteracted by the contiguous position of the Baltic. It is more owing to the clearness of the sky, by which the limited, yet adequate, influence of the sun is fully imparted, than to the nature of the soil, however good, that the Dantzic grain, particularly the wheat, is of a firmer texture than the English; and it is likewise owing to the unequal duration of the sun's influence, or summers, (both skies being about equally clear,) that it is in general fuller and larger than any yielded in the most fertile parts of either of the Canadas.

tellectual excitement is increased* by the *ne plus ultra* of vertiginous motion at the equatorial parts in the circumrotation of the oblate spheroidal body of the earth, and in the *maximum* agitation of the projected waters, independently of winds from the more immediate influence of the moon.

On the 13th of the same month, a little before noon, we crossed the tropic of Cancer in $25^{\circ} 05'$ west longitude, where the mercury was up to 80. From the time of our leaving this remarkable parallel until our arrival at Rio de Janeiro, which happened on the 17th of December, it proved to be between 78 and 91; the latter having taken place the 24th of November at noon, in lat. $6^{\circ} 6'$ north, and long. $20^{\circ} 18'$ west, the wind on the same day having been variable and inclining to a calm. On the night of the 24th, and particularly towards four in the morning, the rain fell in torrents, attended with incessant flashes of vivid lightning, and with reiterated peals of the loudest thunder. It was perhaps fortunate for us that our conductors had been placed.

It is universally known from a repetition of observations, in confirmation of which I beg leave to throw in my mite of testimony, that in the intervals of the vernal and autumnal equinoxes, and for some time before and after, the trade-winds extend as far as 30° , or thereabout, on each side of the equator, where they are about N.E. and S.E., each; and that as the sun advances towards either of the tropics, the trade-wind corresponding to the same, relatively advances beyond its parallel of limitation, and gradually assumes a more northern or southern direction, until at last it is insensibly, or suddenly, or suddenly and violently lost in those fluctuating and frequently raging† winds preserving the atmospheric equilibrium; while, on the other hand, the trade-wind corresponding to the opposite parallel of limitation of greater obliquity to the sun, recedes in a certain degree in the direction of its proximate tropic, and assumes a more easterly direction. The limits of the trade-winds, depending as they do on a variety of causes that involve complicated though certain principles, are extremely uncertain, more particularly the northern. The *Coromandel*, for instance, on her return, in June, had the N.E. trade-wind with slight variations previous and subsequent to her

* The excellent Mrs. Barbauld, however, judiciously as well as emphatically remarks, that

“ ——— Souls are *ripen'd* in our northern sky.”

She doubtless implied the Gallic, and meant the Scottish as well as English sky. With regard to Ireland, I cannot but agree with Bisset, that it or its sky is more frequently the mother than the nurse of genius.

† In eight months out of twelve, strong winds, if not gales, prevail near the Azores.

arrival at 30° N.; after which, she for the most part had permanently, though not in exact succession, the N.E. $\frac{1}{2}$ N. N.E. by N., &c. trade-winds as far as $37^{\circ} 46'$ N.; where, on the 26th of June, at 11 A.M. in $44^{\circ} 00'$ west longitude, not very remote from the Great Bank, she fell in with a moderate breeze from the S.E. by E., that freshened and veered more to the southward towards night. It is likewise known, that at any intermediate parallel between these principal parallels of limitation and the equator, the wind corresponds, under the intervening differences just stated, to some intermediate point between either of these points and the east. Hence, it is evident, that there is an east wind at the equator, or at some intermediate parallel, where the heat is most intense. As the equator is the centre of the sun's declinations, and as the sun is on it twice in the year, and on each of the tropics but once, it is obvious that the equatorial parts ought to be the hottest; but from the difference of densities between the two hemispheres, and from the sun's being about eight days longer in the northern than in the southern hemisphere (besides other reasons, which I shall presently assign), the parallel might be reasonably presumed to be on the northern side; and accordingly it is found, from repeated observations, to be between the 2d and 5th or 6th degree of north latitude. On our way out to Rio Janeiro, I found the temperature between those latitudes higher than that between the same latitudes south, notwithstanding the greater obliquity of the sun's rays at the same time. At the north side of the equator, as far as 7° or 8° , the winds thus counteract each other, and are productive of calms, or light breezes that are continually shifting; a circumstance rarely remarked in corresponding latitudes south.

It will, I trust, not be superfluous here to extend the foregoing comparison concerning the difference of temperature beyond the tropical circles. It appears from Cook's tracks in the southern hemisphere, that the polar ice there extends further beyond the antarctic circle, than what the north polar ice does beyond the arctic circle. Independently, however, of local circumstances, it also appears, that the degree of temperature arising from equal declinations of the sun is in general lower in the same parallels south than north. This, considered chiefly in a scientific and partly in a hypothetical light, may be accounted for, briefly, as follows:

1. We know from the relative positions of the earth in the ecliptic, that the sun is more remote during the southern winter, by about three millions of miles, *et cæteris paribus*, than it is during the northern winter, *et vice versâ*. Hence the diminution of the sun's influence, as it relates to the southern hemisphere, is not only increased in proportion to the increasing obliquity of his rays,

rays, but also in proportion to their increasing distance, during his apparent motion from the perihelion to the aphelion; and, on the contrary, the diminution of the sun's influence on the northern hemisphere, from his increasing obliquity, instead of being further increased by a secession, is rendered less by his continual approach, during his apparent motion from the aphelion to the perihelion.

2. In consequence of the gradual and maximum advance of the perigeum on the ecliptic, since the coincidence of the perihelion and aphelion with the equinoctial points, in the beginning, and in consequence of the progressive motion of the perihelion according to the order of the signs in the same orbit; likewise since the sun is nearly eight days longer in the northern than in the southern hemisphere, and has been about 100* years longer in it, since the beginning, or, strictly speaking, since the omnipotent *fiat* of the projectile force.

3. The abstractive force in the atmosphere during the absence of the sun is greater in proportion than the attractive force of the earth during his presence.

4. The greater collection of waters in the southern hemisphere, which, from the preceding circumstances, indicate the great and continued abstraction of calorific, in the successive and protracted accumulations of ice. The excess of waters in the southern hemisphere, which, by absorbing to a certain depth the matter of heat, admits of a protracted abstraction of it during the long absence of the sun; the sun being nearer to it, in the middle of their summer south, than ours north, by the aforesaid distance; and the increase of the sun's influence, in the continued diminution of his obliquity, being further augmented by his continual approach, during the time of his leaving the aphelion until his arriving at the perihelion, are the principal circumstances relating to the sun's influence on the southern hemisphere, which are comparatively of less force than the foregoing, in relation to his influence on the northern hemisphere.

And hence it is, that islands, although not in very high southern latitudes, are colder, or are impregnated with frost, or covered with snow, to a degree beyond what islands or places in the same or higher latitudes north are. I was informed by two men (in the Coromandel at present) who had formerly belonged to the Emerald South-sea-man elephant-hunter, of 410 tons, Mr. James Walker master,—that on this vessel having touched at the island of Desolation, in the latter end of July in 1811, they had occasion to inter the body of a man, which, on arriving there

* *Plusve minüsve*.—The exact time I leave to such accurate mathematicians to find out as Mr. Keith, the only writer I know of that has exquisitely simplified the ambiguities of oblique-angled spherical triangles.

about two years after, they found as fresh as on the day of interment, so completely protected had the grave been by the congealment of the snow that had fallen, and which, it appeared, had resisted the influence of the sun and equatorial winds. Their long-boat too, which they had no scruple in leaving there, they found safely imbedded in a congealed mass of snow on their return.

In my letter to you from the *Prévoyante*, in the Channel, in 1817, which you deemed worthy of meeting the public eye, although not *slyly* intended for it, I believe I pointed out that the western parts of the northern hemisphere, where I had been, are colder in winter, and more variable in summer, than those places, under the same parallels, in the eastern part of the same hemisphere; and from what I lately heard, as well as formerly read, I am likewise led to think that the air of South America is, on the whole, colder or more temperate than places under the same latitudes in the southern part of the eastern hemisphere, even after the exclusion of the vast sandy and heated deserts in it.

A little after 10 A.M. of Friday November 27th, in latitude 5° north, and long. $20^{\circ} 26'$ west, about half-way between the ship and the verge of the horizon, the ship's head having been S.W. $\frac{1}{2}$ W. I observed on the larboard quarter a large water-spout. It appeared in the form of a long clumsy pillar, with its superior part expanding into a large dense black cloud, from which it seemed to be suspended to about 50 or 60 feet above the level of the sea. The water, I understood, had just ceased to be violently agitated as I got a sight of it. The sides of the spout appeared of the same black colour as the cloud, and the internal part of the body of it of a dusky hue. It might have been about a minute in going up, rather slowly at first, but finally with a motion that was instantaneous.—The following I beg leave succinctly to state as my notions concerning these phænomena.

Electricity is known from experiments to be in a greater quantity in clouds than in the proximate parts of the atmosphere. It is particularly attractive of aqueous vapour, and suspends it after it is condensed. Clouds are therefore a peculiar vehicle of it. Its agency and effects, however, are most striking in those containing the greatest proportions of light and calorific, and are commonly manifested on any abstraction of the latter; and on the contrary, the decrease of temperature not unfrequently noticed after thunder-storms and changes of winds, is the natural result of its presence and its influence. A great proof of its affinity to the matter of heat is, that all bodies become conductors of it when they are made hot. The motions of the clouds from wind are influenced by it, in its motion to restore the equilibrium, when

they happen to contain different proportions of it. As the atmosphere, from its elasticity and motive powers, is more or less in motion, the agency of this extremely subtile fluid manifests itself in water-spouts, as well as in lightning and thunder, and the rest of the great and awful convulsions in nature.

In the densest clouds there is not that degree of density that there is in water on the earth; because electricity, as it becomes intermixed with clouds in a state of vapour or expansion, counteracts intimate cohesion or density by its motion, its subtilty, and its repelling powers, when the contiguous parts, which it necessarily pervades, happen to be similarly electrified. But as, in the motion of the air and clouds, the influence of electricity is neither constant nor uniform in the same or different parts, those adjoining will necessarily gravitate, and the inferior part, by its superior force of gravitation, will at last overcome the suspensory power of the electric fluid, and instantaneously descend, and continue to do so, until the cloud shall be exhausted, or the gravitation diminished, when the suspensory influence will, in consequence, become greater as before, and carry up again, principally by its cohesion and extreme subtilty, what had remained unspent of it.

RIO DE JANEIRO.—A person on approaching Rio de Janeiro at first, is struck with the peculiarity of the appearance of the land, so unlike any place he had ever seen, or, in all probability, imagined before. Straight before him, a number of detached hills, curiously and differently shaped, seem to rear their heads, in wild disorder, out of the bosom of the ocean. Far to the left are seen the high lands of Copacabana—

“ Clad in colours of the air,
Which, to those who journey near,
Barren, brown and rough appear ;”

and likewise the towering Gavia, the summits of which, according to the particular state of the weather, are ever capped with clouds of the fleecy or gloomy hue. Nearer, also in this direction, is seen, somewhat in the form of a cone, inclining towards an adjoining ridge, so as to make something like an elliptical section with it, that enormous rock called, from its implied resemblance, the Sugar-loaf, which is ever the prominent and unerring landmark for mariners; and near which they can anchor their vessels in safety, should the wind and tide prove at any time unfavourable on entering the harbour. Close behind it a congeries of mountainous hills fantastically rises, which bids defiance to the powers of the pencil and description:—and on the right, or Braganza side, is seen, but more in an extreme than lateral direction, a chain of lofty hills, from which, to the northward, high lands with rugged summits take a curvilinear course. Castle-hill (the name of the
first

first of this chain) is the site of the principal telegraph, and has the manifest disadvantage of being in no small degree irremediable to the progress of the sea-breeze in the direction of the town. On approaching nearer the harbour are seen, at a distance beyond the end of it, like an immense black cloud extending in the northern-eastward and southern-westward directions, the Organ Mountains, so called on account of the inequality of their various jutting peaks, and their fancied resemblance to the pipes of the instrument of that name when viewed at a distance. To the left of these, again, is seen afar the alpine *Tengua*, over which are the passes to the mines. And on entering the harbour, the contemplative faculties are called up by the picturesque appearance, exquisite beauty, and striking grandeur of the surrounding scenery. The harbour itself, particularly when viewed from any of the lofty hills contiguous to it, seems to correspond more to the description of a gulf, than to the common termination of two rivers forming a harbour. It abounds with beautiful and attractive bays, that have more the appearance of romantic lakes; and which are happily rendered cool and refreshing during the noontide heats of a vertical sun, by the sea-breeze blowing directly into them, or circuitously through delightful glens or windings, and thereby usually with accelerated motion.

As the force and extended influence of the sea-breeze of any maritime place depend in a great measure on its position, and on the extent and nature of the adjacent land, the wind blowing into Rio de Janeiro (situate as it is on the east coast of America and within the southern verge of the torrid zone) is compounded of the trade-wind and sea-breeze; and accordingly it in general blows with more than refrigerative force. The sea-breeze generally sets in about noon, or occasionally about an hour or two before or after; after which it gradually increases, and continues fresh for about from three to six hours, when it dies away; a perfect calm then ensues, and lasts for some time, frequently until about midnight; at which time the land-breeze springs up, and is succeeded by a short interval of calm, or by an immediate sea-breeze. At other times the land-breeze almost immediately succeeds the sea-breeze; and sometimes an unusual cessation of the one is productive of an irregular suspension of the other; both, however, are still referable to their primary causes.

I have, at different times, noticed the mercury in the thermometer to be influenced by the sea-breeze, without any apparent influx or rise of moisture on the one hand, or dispersion of it on the other. Between the 17th of December and the 23d of January (the interval of our first stay here) the temperature was between 93° or 92°* and 82°; and between the 18th of March and

* On shore it was frequently up to 94° and 95°.

the 24th of April (the interval of our second stay) it was between 85° or 86° and 78° . I never observed it, under any circumstance, to vary more than 4, 5, or 6 degrees within twenty-four hours. The season, however, proved uncommonly dry. I was told that it occasionally varies more than this within that time. I understood from a surgeon resident some time here, that it is in winter so low as 51° : I, on the other hand, learnt from an intelligent merchant*, many years here, who I know has been particularly attentive in this respect, that it is seldom or never lower than 64° . By this gentleman I was likewise informed that the alteration of temperature at the changes of the seasons is so gradual and imperceptible, that the feelings are scarcely ever sensible of the change, except that at night, in winter, a blanket is required with a sheet, the only bed covering used in summer. Again, all unite in admitting that the temperature and weather in winter are congenial to their feelings; a circumstance that indicates the former not to be lower than 60° , or at the very lowest 55° . The rainy seasons generally take place, as in most other countries, in spring and autumn, and from time to time in summer;—the last, as I before remarked, having proved a complete exception.

Yet, after all, were it not for the salutary influence of the sea-breeze daily, its vicissitudes with the land-breeze, the alternate flowing and ebbing of the tides, and the nature of the soil (which is sufficiently implied in part of the foregoing description), the city of St. Sebastian would certainly be a most unhealthy as well as disagreeable place to live in.

In this city are neither rivers nor streams of any kind. The water destined for the supply of it seems on the whole insufficient for this purpose;—springing out of Cocanada, (one of those lofty hills already described as situate to the left of Rio de Janeiro,) it is finally conducted thither through an aqueduct about four miles in length, from which it discharges itself through three ordinary fountains; where the poor unfortunate negroes, doomed as they are to perpetual and rigid servitude, anxiously wait night and day to be barely supplied in rotation, especially in dry parching weather, when the body of this current, small even in the wettest season, becomes still considerably diminished.

In consequence of the want of sewers, the excrementitious and other matter (necessarily produced by such an immense population as St. Sebastian notoriously contains) is, with the exception of about 150 yards from both sides of what is termed the Palace, thrown openly near the water's side along the whole extent of the town, and principally in those parts corresponding to the two market-places in it. Hence, and from the influence of the very

* Mr. W. Pearson, the copartner of Messrs. Seaton and Plowes, of a first-rate firm in Rio de Janeiro.

high temperature here, and likewise the immense quantities of animal and vegetable matter continually rendered putrid by it, offensive exhalations continue to exist, that affect, particularly at night, the sense of smelling in the strongest degree, and that occasionally extend even to ships in the harbour after the land-breeze has sprung up.

[To be continued.]

XXI. *Experiments on the Gas from Coal, chiefly with a view to its practical Application.* By WILLIAM HENRY, M.D. F.R.S. &c.*

THE chemical properties and composition of the gas from coal formed a principal object of two different series of experiments, the results of which I laid before the public many years ago. The first of these communications, entitled “Experiments on the Gases obtained by the destructive Distillation of Wood, Peat, Pit-coal, Oil, Wax, &c. with a view to the Theory of their Combustion, when employed as Sources of artificial Light,” appeared in Mr. Nicholson’s Philosophical Journal for June 1805 †; and the second memoir was published in the Transactions of the Royal Society for 1808.

By the first train of experiments, I endeavoured to derive, from a careful analysis of the compound combustible gases, a measure of their illuminating power, admitting of more exact appreciation than the optical method of a comparison of shadows. The one, which I was led to propose as the most accurate, and which I still think entitled to preference, was the determination of the quantities of oxygen gas consumed, and of carbonic acid formed, by the combustion of equal measures of the different inflammable gases; that gas having the greatest illuminating power, which, in a given volume, condenses the largest quantity of oxygen. The average results of a great variety of experiments were comprised in the following table.

Kinds of gas.	Oxygen gas required to saturate 100 measures.				Carbonic acid produced.
Pure hydrogen	50				
Gas from moist charcoal . .	60	..	..	..	35
wood (oak)	54	..	..	..	33
dried peat	68	..	..	..	43
cannel coal	170	..	..	..	100
lamp-oil	190	..	..	..	124
wax	220	..	..	..	137
Olefant gas	284	..	..	..	179

* From the Memoirs of the Literary and Philosophical Society of Manchester, vol. iii. Second Series. † 8vo Series, vol. xi. page 65.

In the same essay I maintained an opinion which, on the most mature consideration, I see no reason to change; that the great variety of gases evolved by the destructive distillation of inflammable substances, do not constitute so many distinct species, but are mixtures of a few, the nature and properties of which were before ascertained. It will contribute to render what follows more intelligible, if a brief account be given of those gases of known composition, the mixtures of which, in various proportions, compose, according to this view, all the observed varieties; and I shall make their comparison under a form best adapted to illustrate their practical application.

1. **HYDROGEN GAS.**—This is the lightest of all known gases, its specific gravity, that of atmospheric air being taken at 1000, being about 73. As ordinarily procured, by the solution of iron or zinc in diluted sulphuric acid, it contains impurities which give it a disagreeable smell; but well purified hydrogen has little if any odour. It burns with a pale and feeble flame, not at all suited to artificial illumination.

	grains.	Product of its combustion.	grains.
The cubic foot weighs about	40		
Consumes half a cubic foot			
of oxygen	300		
	340	Water	340

2. **CARBURETTED HYDROGEN** has been shown to constitute the gas of marshes and the fire-damp of coal-mines. In these natural forms, it is contaminated with a small proportion of carbonic acid, and a larger one of azotic gas, but appears to be free from all other impurities. It is proved to be a definite compound of hydrogen and charcoal without any oxygen. It is lighter than common air, in the proportion of about 600 to 1000; it has very little odour; and burns with a flame greatly surpassing that of hydrogen in density and illuminating power.

	oz. dr.*	Products.	oz. dr.
A cubic foot weighs	0 12	1 cubic foot of carb. acid	1 13
Consumes 2 cubic			
feet of oxygen ..	2 10	Water	1 9
	3 6		3 6

3. **CARBONIC OXIDE** is rather lighter than common air. It contains no hydrogen, and is purely a compound of charcoal and oxygen, the latter being in just half the proportion which is required to constitute carbonic acid. It burns with a feeble blue light.

* The avoirdupois ounce of 437½ grains, or 16 drachms, is to be understood.

	oz.	dr.	Product.	oz.	dr.
A cubic foot weighs	1	3			
Consumes $\frac{1}{2}$ a cubic foot					
of oxygen	0	11			
	1	14	Carbonic acid	1	14

4. OLEFIANT GAS, or BI-CARBURETTED HYDROGEN.—This has been demonstrated to be a compound of nearly 85 by weight charcoal, and 15 hydrogen, without any oxygen. It is a little lighter than common air, viz. in the proportion of about 974 to 1000. It surpasses all other gases in the brightness and density of its flame. Its name was originally derived from the property which it possesses, of being speedily and entirely condensed, by rather more than an equal volume of chlorine gas, into a liquid resembling oil in appearance, but since shown to approach more nearly to the nature of ether.

	oz.	dr.	Products.	oz.	dr.
A cubic foot weighs	1	3	2 cubic feet carb. acid	3	10
Consumes 3 cubic					
feet of oxygen	4	0	Water	1	9
	5	3		5	3

Olefant gas I found to be one of the products of the distillation of oil and of bees' wax, and was led, therefore, to suggest, that the wick of a lamp or candle, surrounded by flame, is to be considered as a bundle of ignited capillary tubes, into which the melted inflammable matter is drawn, and there resolved, not into a condensable vapour, but into olefant and carburetted hydrogen gases. In the gas from coal, also, I detected the presence of olefant gas, by the test of the action of chlorine.

In the second series of experiments*, I submitted to distillation, on a small scale, various kinds of coal, from different parts of the kingdom. The aëriiform products, at different stages of the process, were kept apart, and were separately analysed. From coal distilled in small iron tubes or retorts, which, when filled, were placed at once in a low red heat, small quantities of sulphuretted hydrogen and carbonic acid gases came over at first, in mixture with the other gases, but in a gradually diminishing proportion, till at length, in the last products, they were not discoverable at all. The production of olefant gas observed the same order, and a gradual diminution took place, as the process advanced, in the combustibility of the gas, as determined by its requiring less and less oxygen for saturation. A great variety was

* Phil. Trans. 1803, page 292.

ascertained to exist in the quality of the gas from different kinds of coal; that from Wigan cannel holding the highest rank in illuminating power, and that from the stone coal of South Wales the lowest.

Since the period when the second of these papers was published, the use of artificial gases, as a source of light, has been rapidly increasing in this, and, I believe, in other countries, and promises to attain an extent and importance sufficient to justify any labour that may tend, however remotely, to its improved application. It has frequently happened, of late years, that I have been requested by the proprietors of large manufactories lighted by gas in this neighbourhood, to give an opinion on practical points, respecting some of which I felt myself incompetent to decide, from the want of the necessary data. It is to supply these data, that I have once more returned to the investigation of the subject. The objects which I have had it in view to determine by the following course of experiments, are, whether, on the large scale of manufacture, there is a decline in the value of the aëriform products of coal, from the beginning to the end of the distillation, similar to that which takes place on a small scale;—at what stages of the process those gases, which may be considered as impurities, are chiefly evolved; and whether they are essential or accidental products;—whether the method of removing the sulphuretted hydrogen and carbonic acid gases by quicklime, which I suggested in the second memoir, is adequate to the complete purification of coal gas;—whether this purification is attended with any loss of that portion of the gas which, on account of its superior illuminating power, it is desirable not to remove;—and, if such a loss should be found to ensue, whether it may not be avoided by some modification of the purifying process. In determining these points, I was indebted for the necessary supplies of gas to Mr. Lee, at whose extensive manufactory the principal facts were ascertained, that formed the basis of the first accurate calculations respecting the œconomy of gas from coal.*

On the Quality of the Gas, at different Stages of the Distillation.

The gas which I first submitted to experiment was obtained from Wigan cannel coal, a substance preferred in this neighbourhood as affording aëriform products, which, both by their quantity and quality, more than compensate its higher price†. The retorts are charged while red hot with this substance, and indeed

* See Mr. Murdoch's "Account of the Application of the Gas from Coal to œconomical Purposes," Phil. Trans. 1808, page 124.

† About a shilling per cwt. of 112 lb. or 13½d. delivered in Manchester.

are never suffered, during the whole of the winter season, to fall below the temperature of ignition. The gas was collected in a bladder furnished with a stop-cock, which was fixed into an opening in the pipe between the retort and the tar-pit. It was taken at this place, in order to avoid contact with water, and admixture with any atmospherical air, that might accidentally remain in the gasometer. Wishing to examine the gas in a perfectly recent state, and finding it impossible to make the necessary experiments with sufficient accuracy in a shorter interval, I was obliged to be satisfied with procuring it every other hour. In this place, I shall only state the general results; and I shall describe, in a subsequent part of the paper, the methods of analysis, in order that other persons, who may choose to compare my experiments with their own, may conduct them under equal circumstances.

By the expression *impure gas*, is to be understood, the gas precisely in the state in which it was collected from the retort; and by *purified gas*, the same product after being freed from carbonic acid and sulphuretted hydrogen by solution of pure potash, applied in very small quantity, relatively to the volume of the gas, and with the least agitation adequate to the effect.

TABLE I.

Showing the Quality of Gas from 1120 lbs. of Cannel, at different Periods of the Distillation.

Hours from the commence- ment.	100 measures of im- pure gas contain of		100 m. purified gas consist of			100 m. of purified gas	
	Sul. hyd.	carb. ac.	Olef. gases.	other infl.	az.	cons. oxyg.	give carb. acid.
$\frac{1}{2}$ an hour	$0\frac{1}{2}$	$5\frac{1}{2}$	16	64	20	180	94
1 hour ..	3	$3\frac{1}{2}$	18	$77\frac{1}{4}$	$4\frac{3}{4}$	210	112
3 hours ..	$2\frac{1}{2}$	$2\frac{1}{2}$	15	80	5	200	108
5 hours ..	$2\frac{1}{2}$	$2\frac{1}{2}$	13	72	15	176	94
7 hours ..	2	$2\frac{1}{2}$	9	76	15	170	83
9 hours ..	$0\frac{1}{2}$	$2\frac{1}{2}$	8	77	15	150	73
$10\frac{1}{2}$ hours ..	0	2	6	74	20	120	54
12 hours ..	0	$0\frac{1}{2}$	4	76	20	82	36

Excluding from the calculation the azotic gas, with various proportions of which the products were contaminated, the following table shows the quantity of oxygen gas consumed, and of carbonic acid produced, by the really combustible part of the gas.

TABLE

TABLE II.

Showing the Quality of the really combustible Part of the Gas, at different Periods of Distillation.

	Take oxygen.				Give carb. acid.	
100 measures of						
half hour's gas	..	225	..	..	..	118
1 hour's gas	..	220	..	..	..	117
3	..	210	..	..	..	114
5	..	206	..	..	..	108
7	..	200	..	..	..	98
9	..	176	..	..	..	85
10½	..	150	..	..	..	70
12	..	103	..	..	..	45

The next set of experiments was made on gas from common coal, got at Clifton near Manchester, and of fair average quality.

TABLE III.

Showing the Quality of the Gas from 1120 lbs. of common Coal, at different Periods of the Distillation.

	100 m. of im- pure gas con- tain		100 measures of purified gas.			100 measures purified.	
	sulp. hyd.	carb. acid.	olef. infl. gases.	other az.		cons. oxy.	give carb. acid.
1 hour's gas	3	3	10	90	0	164	91
3 hours do.	2	2	9	91	0	168	93
5 hours do.	3	2	6	94	0	132	70
7 hours do.	1	3	5	80	15	120	64
9 hours do.	1	2½	2	89	9	112	60
11 hours do.	1	1	0	85	15	90	43

Exclusive of the azote, with which the three last portions of gas were mingled, they consumed oxygen and gave carbonic acid as follows. The seven hours gas in this instance, as sometimes happens from irregularities of temperature, was more combustible than that collected two hours sooner.

	Consumed oxygen.	Gave carb. acid.
100 m. of 7 hours gas	.. 140	 75
9 hours	.. 123	 66
11 hours	.. 106	 50

A comparison of the results exhibited in the third table, with those of the distillation of cannel coal, is greatly in favour of the latter substance as a source of light. This will appear most distinctly, by setting against each other the proportions of oxygen, which are consumed by the gases evolved from the two substances at equal times from the commencement.

TABLE IV.

Comparative Table of the Qualities of the Gases from Wigan Cannel, and from common Coal, at equal Times from the Commencement of the Distillation.

	Oxygen consumed by 100 m. can- nel gas.				Oxygen consumed by 100 m. of Clifton coal gas.			
1 hour's gas	..	..	220	..	..	..	164	
3 hours	..	..	210	..	..	..	168	
5 hours	..	..	206	..	..	..	132	
7 hours	..	..	200	..	..	..	140	
9 hours	..	..	176	..	..	..	123	
11 hours	..	..	150	..	..	..	106	

It appears from these experiments, that the gas from cannel has, in an equal volume, an illuminating power about one-third greater than that from coal of medium quality. The quantity, also, from the former substance, exceeded by about one-seventh that obtained from coal, distilled under precisely similar circumstances; 3500 cubic feet of gas having been collected from 1120 pounds of cannel, and only 3000 cubic feet from the same quantity of coal. The whole product of one distillation of cannel, mixed together in a gasometer, was of such quality, that 100 measures required for combustion 155 measures of oxygen gas, and gave 88 measures of carbonic acid. But as the gas was contaminated with 15 measures of azote in every hundred, the oxygen, required for saturating 100 measures of the really combustible part of it, may be stated at 195; and the carbonic acid produced at 110. It may be necessary to observe, that in comparing the value of gases produced from different kinds of coal, or from the same kind of coal differently treated, it is not enough to determine the *quantity* of aëriform products; and no satisfactory conclusion can be drawn respecting the relative fitness of any variety of coal for affording gas, or the advantages of different modes of distillation, unless the *degrees of combustibility* of the gases compared be determined, by finding experimentally the proportion of oxygen gas required for their saturation.

The results expressed in the first table, when contrasted with those which I formerly obtained by the destructive distillation of small quantities of coal, present several circumstances of disagreement, as to the quality of the products at different stages of the operation. In small experiments, the sulphuretted hydrogen and carbonic acid gases were evolved only at the early stages of the process; and sulphuretted hydrogen, especially, could not by the nicest tests be discovered in the last products of gas. On the large scale, both these gases continue to be evolved throughout the whole operation, though in greatly diminished proportion to-
wards

wards the latter end. Even in the advanced stages of large distillations, the presence of sulphuretted hydrogen in coal-gas may be traced by the proper test, though not in a quantity that admits of being easily measured. The test which I used for some time was the white oxide of bismuth, for which I afterwards substituted white lead, ground with a little water to the proper consistence, and spread by a camel's hair pencil on a slip of card. This was secured by a small pair of forceps fixed in a cork, by means of which the slip of card could be placed in a jar or bottle of the gas, and kept there for some time. By experiments on artificial mixtures, I found that a cubic inch of sulphuretted hydrogen, diffused through twenty thousand cubic inches of common air, distinctly affected the test, which it changed to a light yellowish or straw colour. By mixing sulphuretted hydrogen with various proportions of common air, I prepared coloured cards of a variety of shades, which served as standards of comparison for judging of proportions of sulphuretted hydrogen in coal-gas, which were too minute to be accurately measured.

In the small experiments made several years ago, I never found, in the early products of gas from cannel coal, a proportion of olefiant gas at all approaching that which is noted in Table I. and its quantity in small distillations rapidly decreased, until in the latter products it could be no longer traced at all. The method of analysis, which I formerly employed, led me, however, as I have lately discovered, to under-rate the proportion of olefiant gas, and to over-estimate that of sulphuretted hydrogen. But making due allowance for this error, the superiority of the products of large operations, so far as respects olefiant gas, still exists, and is confirmed by comparative experiments on a small scale which I have lately made. Thus it appears from Table I. that even after twelve hours continuance of the process, olefiant gas still constitutes 4 per cent. of the gases evolved from cannel. The other inflammable gases, also, when obtained in large quantity, are more uniform in quality, and possess, towards the close of the process, much greater combustibility and illuminating power, than when procured in small experiments. This superiority is obviously dependent on the greater facility of preserving an uniform temperature, in all chemical processes which are carried on upon a scale of magnitude.

The temperature to which the coal is subjected, must necessarily be a point of the greatest importance to the quantity and quality of the æriform products; for while too low a heat distils over, in the form of a condensible fluid, the bituminous part of the coal which ought to afford gas, too high a temperature, on the contrary, occasions the production of a large relative proportion of the lighter and less combustible gases. It would be a
great

great step in the improvement of the manufacture of coal-gas, if the whole of the hydrogen could be obtained in combination with that proportion of charcoal which constitutes olefiant gas ; and it is satisfactory to know, that no impediment to this arises out of the proportion of the hydrogen and charcoal present in coal. If this object be ever accomplished, it will probably be by the discovery of means of uniformly supporting such a temperature as shall be adequate to the production of olefiant gas, and shall never rise above it ; and some probability of success is perhaps derivable from the fact, that M. Berthollet, by the careful decomposition of oil, which in my experiments afforded a mixture of gases, succeeded in obtaining olefiant gas in a state of purity*.

With the view of ascertaining how low a degree of heat is adequate to the production of gas from coal, I placed a small iron retort, containing cannel, in melted solders of various composition, without obtaining more than the common air of the vessel. The retort charged with fresh materials, was then immersed in melted lead ; but after expelling the common air, no more than a few bubbles of gas came over, and that only when the lead, by being kept over the fire, had acquired a temperature about its fusing point. On restoring this temperature by adding fresh metal, the evolution of gas was always suspended. I placed also one of Mr. Wedgwood's pyrometer pieces in contact with a retort which was at work at Mr. Lee's manufactory, and which showed only a dull red or blood-coloured heat ; but, after remaining in that situation half an hour, a contraction of barely one degree of the scale had taken place. This temperature, however, I suspect is rather too low, and has a tendency to distil over too much tar, and consequently to produce less gas than might be obtained by a degree of heat somewhat higher. The best adapted temperature will probably be found to vary with different kinds of coal ; and I have been prevented from ascertaining it with respect to cannel, by the inconveniences that would arise from disturbing the regular arrangements of a large manufactory. From some experiments of Mr. Brande, it appears that the sudden application of the requisite heat evolves from coal much more gas, than the gradual heating of a cool retort up to the point of ignition†.

In the experiments upon gas from Wigan cannel, the results of which are comprised in the first table, azotic gas was found in all the acriform products, from the beginning to the end of the

* *Mémoires de la Soc. d'Arcueil*, ii. 84.

† *Journal of Science*, vol. i. page 75.

operation. But in experiments on the gas obtained at other times from the same substance, no appreciable quantity of azotic gas could be discovered till after the sixth hour of the process, when it began to appear, and progressively rose to 20 parts in the hundred. Of this purity of the early products from azote, and appearance of it in the latter ones, Mr. Dalton was an eye witness on one occasion, when he was so good as to co-operate with me; and I had afterwards repeated opportunities of verifying the fact. With the view of ascertaining whether the azote found its way from the atmosphere into the distilling vessels, I subjected 100 grains of cannel coal to heat in a glass retort, the capacity of whose body and neck did not together exceed $1\frac{1}{4}$ cubic inch. Besides a portion of gas which was lost, 50 cubic inches were collected, which, on careful analysis, were found to contain 5 cubic inches of azotic gas. Of these only one cubic inch can be traced to the common air present in the retort at the outset; and the other 4 cubic inches must have been furnished by the coal itself.

It is resonable indeed to expect, that a substance like coal, which affords ammonia under some circumstances, should, under others, yield the elements of that alkali in a detached state; and the reason, why azote is for the most part not to be found in the gas which is first evolved, is, that at a low temperature, that element unites with hydrogen, and composes ammonia. But when the contents of the retort, which, for some time, have been kept comparatively cool by the escape of condensable fluids, become more intensely heated, ammonia is either not formed, or, if formed, is decomposed again into azotic and hydrogen gases, both of which may be traced in the aëriform products of the advanced stages of distillation. As a matter of practice, it is certainly desirable that the azote existing in coal should enter into the composition of a condensable fluid, rather than that it should escape in a gaseous state; for it is an impurity which, when once mingled with the combustible gas, cannot be removed by any known method, and must materially impair its illuminating power. That such an effect must result from its presence, may be inferred from the experiments of Sir H. Davy, who found that an explosive mixture of carburetted hydrogen and common air was deprived of its combustibility by being mixed with one sixth of its bulk of azotic gas*.

[To be continued.]

* On the Safety-lamp, page 30.

XXII. *Free Remarks on the Geological Work of Mr. GREENOUGH.* By Mr. JOHN FAREY, Sen. Mineral Surveyor.

To Mr. Tilloch.

SIR, — MR. GREENOUGH, who was the first President of the Geological Society of London, and now again fills that situation, has at length appeared before the public in the character of an Author: his Work, entitled, “A Critical Examination of the first Principles of *Geology*,” follows very closely in the steps of that of Dr. Kidd*, in labouring excessively to show, that scarcely any thing of *real knowledge* yet exists, respecting the *composition*, the *structure*, and the past *history* of the crust of the *Earth*!! On scarcely any of the numerous points of Geological Theory or the general *Inferences* from Geological Facts, which Mr. Greenough’s Work brings under review, does *he* seem able to have made up an opinion, and for want of which, his readers are throughout bewildered, by a mass of contradictory extracts from those Authors (certainly not a few) whom Mr. G. (in imitation of Dr. K.) in a somewhat arbitrary, and sometimes, as it appears to me, in an unfair manner, selects, as *authorities* in Geological matters: and too often also, the Author’s own remarks are found at variance with each other.

In thus freely stating my opinion on the general character of Mr. G’s Work, as appears to me after very carefully and repeatedly perusing it, I by no means wish or intend to deny it the merit, of containing a great many *original and useful local Observations*, and also several *Inferences* and *Remarks*, which had not previously been published, and for performing which service to Geology, no one in England, feels more grateful to Mr. G. than myself: although on the whole, I cannot consider his conduct fair, or his Work as likely to advance, but rather to retard, the march of Geological knowledge.

The subject of Organic Remains, and of *Fossil Shells* in particular, appearing to me of paramount importance, amongst Geological phenomena, I shall on the present occasion, principally confine my remarks to the manner in which Mr. G. has handled some of this part of his subject, which is found either incidentally or more formally introduced, at pages 27, 34, 179 to 184, 203, 207, 215, 220 to 225, 227, and 284 to 304.

In page 284 Mr. Greenough, in evident allusion to the question which has often been agitated in your pages, viz. how far Mr. *W. Smith* is to be considered as a *discoverer* of the connec-

* On this Work I offered some strictures, in your 45th vol. p. 338; see also vol. 52, p. 184 Note.

tion now so well proved to exist, between *particular Beds* or *laminæ* of the *Strata*, and *particular Species* or varieties of *Shells* or other *Organic Remains*, which are found imbedded therein, and as the first who actually *used and taught* this mode of *identifying, mapping and tracing the Strata*, remarks as follows: viz. "An opinion has for some time past been entertained in this country, that *every Rock* has *its own Fossils*."

Before I proceed to remark on the Extracts, and mention of the Writings of former English Naturalists, which follow in Mr. G's work, and by which he wishes to appear to prove his position above quoted, I will remark on the loose manner in which the two material parts thereof are defined, that are marked with italics: leaving thus his proposition open to the showing, as in some of the following extracts is attempted to be done, viz. that each "different stone," that is each *mineral species* of Stone (without regard to its place in the series of *Strata*), "yield quite different sorts or *species of Shells*," and that the supposed relation subsists, between *mineral* and *animal Species*, instead of the relation which Mr. Smith and myself contend for, viz. between the successive periods or *eras of deposition* of the particular *Beds*, and the particular species or varieties of *Animals*, which, at or immediately prior thereto, existed in the water, on the bottom of which the *Beds* in question were formed.

Respecting Mr. G's Extract in page 284, from Dr. Lister's Paper in the Philosophical Transactions, No. 76 (or Lowthorp's Abr. II. 425) it is material to take into consideration, the connection of the same with the passage which immediately precedes it, as follows, viz. In "our *English inland Quarries*, I am apt to think, there is no such matter as *petrifying of Shells* in the business: But that these *Cockle-like Stones* ever were, as they are at present, *Lapides sui generis*, and never any part of an *Animal*. It is most certain that our *English Quarry-shells* (to continue that abusive name) have no parts of a different Texture from the *Rock* or *Quarry* where they are taken; that is, that there is no such thing as *Shell* in these *Resemblances of Shells*, but that *Ironstone Cockles* are all *Iron-stone*: *Lime* or *Marble*, all *Limestone* or *Marble*; *Sparre* or *Christalline-shells*, all *Sparre*, &c. and that they were never Part of an *Animal*. My reason is, that *Quarries* of different *Stone* yield us quite different sorts of *Species of Shell*," (not *Shells*) and so on, nearly as in the 9 following lines of Mr. Greenough's Extract, at top of page 285; which *thus prefaced*, as in literary justice they ought to have been, will not I think be judged by impartial persons, to amount to much, against Mr. Smith's claim, as above stated, and in your 51st vol. p. 177.

The next paragraph in Mr. Greenough's Work, still alluding to Dr. Lister, is as follows, viz. "The same Writer *followed the course*
of

of the *Chalk-marl* over an extensive tract of country, *by mere attention to its Fossils*," but for reasons best known to himself, Mr. G. omits here, the reference, to the work to which he alludes, in making this important assertion. After reading through all Dr. Lister's Papers in the Phil. Trans., without discovering any such thing, I was induced to look into his Work entitled "*Historiæ Animalium Angliæ*," wherein, at p. 228, at the end of the description of the small Belemnite, engraven in his 32d title or figure, are these words, viz. "*Locus. Hunc lapidem plurimis in locis apud nos quàm copiosissimè inveni: at perpetuo in terrâ rubrâ ferreâ, sive ea mollior gleba, sive saxeâ sit. In all the Cliffs, as you ascend the Yorkshire and Lincolnshire Wooldes for above 100 miles in compas: as at Spiton, Lawnsborough, Castour, Tedford, Calkwell:*" which doubtless is the passage, to which Mr. G. alludes, and the first part of which, a learned naturalist of my acquaintance*, renders thus, viz. "This stone is found very abundantly in many places amongst us, in a red ferruginous Earth, either in softer or in more stony masses. In all the Cliffs," &c. as above.

On which, I beg to ask Mr. Greenough, how he can make out, that the *red ferruginous Earth*, here alluded to by Dr. Lister, at Specton Cliff, Londsborough, Caistor, Cawkwell, and Tetford (as the places are now called), is the *whitish, or blueish or greenish-gray* "*Chalk-Marl*"†, rather than the "*Brick-earth?*" or micaceous blue Marl (exposed and oxidated) p. 13, of Smith's "*Strata Identified*," wherein such small Belemnites, are found, and described. And next, I will take the liberty of saying, that Dr. Lister does not appear to me, either to have accomplished, or to have had in view, the tracing or following of any particular Stratum, by means of these small Belemnites, by reason that he never mentions the same, or speaks of its relation to the Chalk Strata near adjacent and above it in the Series; but intended merely to explain more particularly, by the mention of *Cliffs* at the edge of the Wold Hills, *the situations in which he had found these Belemnites*. It is however, a singular circumstance, that this passage, and this only, should be given in English, in Dr. Lister's Latin

* At my request, the same kind friend, looked carefully through all this work of Dr. Lister's, in order to inform me, whether there are any other passages therein, favourable in any way to the assertions Mr. Greenough has made, regarding Dr. Lister's knowledge and use of Fossil Shells, in tracing or following Strata, by their means?: and his answer is, "It does not appear that Dr. Lister, either traced the Strata by the Shells, or the Shells by the Strata; it also appears, that he often confounded several Species under one Title."

† Derbyshire Report, I. 112; a name, which since 1811, Mr. Smith appears to have dropped, and included this Stratum in the "*Green Sand*," of his Map and Publications.

Work, and further, that it should have been marked by italics! ; and in order to show my readiness, to the giving of every degree of publicity to what Dr. Lister did, with regard to Fossil Shells, I have been at the pains to make out, as I hope, with tolerable certainty, what are the Situations of all the *Places*, in modern Maps, to which Dr. Lister refers in this work; and by means of Smith's Map, joined with my own knowledge of the local situations and extents of the several Strata, to assign each Shell its place, in a Stratigraphical System: to which, I have added references to Mr. Sowerby's Mineral Conchology, wherever he appears to have described the same species of Fossil Shell, and I now send you the same, and shall be glad to see it inserted, following this, in your Philosophical Magazine.

Mr. Greenough's next passage, in page 285, is as follows, viz. "Mr. Strange traced the *Gryphus* from the lower part of Monmouthshire and Purton Passage, through Gloucestershire, Worcesterhire, Warwickshire, and Leicestershire, occupying in these counties, as in Northamptonshire, the lower parts under the hills," and by a Note on this passage, he refers to the "*Archæologia*," vol. vi. p. 36. I have from the volume last mentioned, carefully extracted all which Mr. Strange says, on the subject of Organic Remains, and have sent the same herewith, in hopes that it may be recorded in your Work, following my abstract of Mr. Lister's Work, above mentioned. Mr. Greenough, besides having introduced here, the mention of *Northamptonshire*, which Mr. Strange does not mention (and wherein there is no part of the range of the *Lias* Gryphites, *Gryphæa incurva* α , or *G. obliquata*, Min. Conch. t. 112), he omits Mr. Strange's mention of the *Gravel*, in which he found some, and perhaps several of his Gryphites; and, almost without doubt, these *Gravel* Gryphites belonged to some of the higher stratigraphical localities of this genus of Shells, which are mentioned, P.M. liii. p. 124, or to others, and not to the *Lias* Strata, whereon Mr. Strange's description sets out, in Glamorgan and Monmouth Shires: and hence I think it fair to conclude, that Mr. Strange, had not traced any Strata through distant places, "by mere attention to their Fossils."

Mr. Greenough's two following quotations, from the *Journal de Physique*, seem little to the purpose in question, because, appearing to me merely having in view, the supposed relation between *mineral* and *animal* Species, as before mentioned.

The Rev. A. Catcot, in page 161 of his "Treatise on the Deluge," having spoken of thick and massive Rocks, subjoins a Note which begins as follows, viz. A thick Rock or "single Stratum, is divided into a great number of lesser Strata or small Layers, which will be easily distinguishable from each other, either by their colour, depth, thickness, or more remarkably by their contents, or the Fossil bodies they contain, one layer abounding with one species

of Shells, another with a different," &c., as Mr. Greenough has the remainder of the passage, in p. 286; but who, by omitting this necessary introduction to his Extract, has contrived to keep it out of view of his Reader, that Mr. Catcot, instead of speaking of *the means of identifying the same Stratum or Bed in a different and distant part of its course* (which is the essence of Mr. Smith's claim on this head) was merely speaking of the means of separating a thick Rock, *in some one place*, into its component Strata or Beds.

The extract from the last scientific Letter or Paper which the late Mr. *William Martin* of Macclesfield wrote, before his death, (and which his friends afterwards sent to be inserted in your Magazine, vol. xxxix. p. 81) has very unfairly towards Mr. Smith, (before he had been mentioned, except on a different account in p. 150, as I shall further mention) been introduced, without the mention of the circumstance, which Mr. Martin candidly acknowledges, viz. of *his having omitted to mention in his printed Work*, the connection of *each species of Shell* that he had described, with the *particular Stratum* imbedding the same; Mr. G. well knowing, that Mr. Martin had been led to write the passage which he has transferred to his Book, *in consequence of a particular communication*, made by me to Mr. Martin, of *Mr. Smith's discoveries*, and of my applications of the same, to the Strata and places, from whence Mr. M's Specimens had been collected: after the last of his Works, except this Letter, had been printed off: see P.M. vol. liii. p. 113.

I come now to the short mention which Mr. G. has allowed himself to make of *Mr. Smith*, on the subject of *Fossil Shells*, in p. 287; where, apparently for no other reason than to avoid telling the world, *that Mr. Smith has published* (besides the quarto Memoir explaining his Map) the greater part of *two express works on Fossil Shells* (besides having deposited his original Specimens in the British Museum, which had been almost entirely collected in the last century)—I say, for avoiding saying so much, Mr. G. has *descended so far as to say*, that Mr. Smith's specification of "a variety of Fossils, by which the Strata of England may (in his opinion) be identified," was made, "in a table attached to his *Geological Map*."!!

I have already alluded, to the only other instance throughout Mr. G's Work, wherein *Mr. Smith* is mentioned, which relates, to his discovery, that *the known Alluvia*, that is, such water-moved masses of Strata, as, by containing *known species of Organic Remains* (and being frequently also, of a known substance) which can be matched to *the particular Strata*, from whence they were torn, *have all* (with the exception of some thin and light masses) *been moved in one direction*, that is, from the SE to the NW, or nearly so: and here again, Mr. G., as it appears to me, only

for the propose of *avoiding mentioning* Mr. Smith's "Strata Identified" (p. ii) or his "Stratigraphical System" (p. ix), professes, *not to know Mr. Smith's reasons*, for the opinion which he ascribes to him: notwithstanding, that I have myself, years ago, explained those reasons to him, and have repeatedly published the same in your Work, with confirmatory testimony from my own experience: see vol. xxxv. p. 135, vol. xlii. p. 253, vol. xliii. p. 125 Note, &c.

One thing has considerably surprised me, and all others with whom I have conversed, who had read Mr. Greenough's Book, viz. that not a word or allusion is found therein, to the *Map* ("begun and altogether made on *Wernerian principles*," see p. 337 of your xlvth volume, and vol. lii. pp. 184 and 185 Note), unfounded reports concerning which Geognostic Map, have so long, and so unjustly been played off, against the reputation and sale of Mr. Smith's original Map of the Strata.

I shall have already trespassed too much on your pages, to allow me to mention herein, more than one thing in Mr. G's Book which concerns myself, and that is, to point out, that the words marked with inverted commas in p. 156, and positively ascribed to Mr. Hutchinson, are not his words!, but my words, taken from page 123 (and its Note) of my Derbyshire Report: and further, that Mr. Greenough well knows, that in 1806, I made the important discovery (in Surrey and Sussex, regarding the *Denudation* of the Weald District) which is here alluded to, and gave him, soon after, a *manuscript Section* of the Country between London and Brighton, for explaining the same (P.M. vol. xliii. p. 120); and that my verification and extension of the same discovery, throughout the County and vicinity of Derby, was made, and the same published in Dr. Rees's *Cyclopædia*, and in my *Report*, two years before the time, that either he or myself heard, of the discovery or of the Work of Mr. Hutchinson, or, that of Mr. Catcot, which were brought from Oxford (as I understood) in order to dispute my claims to the first discovery of Denudation, and were lent to me by Mr. Greenough, in June 1813. Although until now, as Mr. G. has been silent to the discoveries of Mr. Hutchinson (who at the beginning of the 18th century was employed by Dr. Woodward in forming his Museum, see Dr. Rees's *Cyclopædia*), and those of his pupil the Rev. A. Catcot, I have let slip no opportunity, of referring to their Works, and doing my endeavours, towards making them more generally known (see vol. xliii. p. 189, vol. xlii. p. 255, &c.); and, in case it would meet your approbation, to insert the same, I would send you the Extracts, which I made from these Authors' Works, in 1813, of all the Geological passages that they contain, which appeared to me new, or important.

I am, your obedient servant,

Howland-street, Aug. 1, 1819.

JOHN FAREY Sen.

A *Stratigraphical* or *Smithian* Arrangement of the Fossil Shells which were described (in Latin) by *Martin Lister*, in 1678, in the 3d Tract of his "*Historiæ Animalium Angliæ*," occupying there 78 pages of very small quarto, with 4 plates, containing 64 figures of Shells. By Mr. JOHN FAREY Sen., Mineral Surveyor.

ALLUVIA, or moved Rubble, and smaller ruins of Strata.

Grantham. } fig. 46, (Plott's Oxfordshire, tab. 4, fig. 8) *Conchites anomius*, &c. p. 240.
Gunnerby. }

Ditto. . . . f. 57, *Pectunculites anomius trilobus*, &c. p. 249.

Keighley. . . . f. 55, *Pectunculites subspæricus*, &c. p. 247.

Upper, or flinty CHALK.

Newton-Grange. f. 26 (Plott, t. 2, f. 12), *Echinites è lapide Selenite*, &c. p. 223.

Norfolk County. f. 19, *Echinites orbiculatus, depressus*, &c. p. 220.

South of England. f. 18, (Plott, t. 2, f. 13) *Echinites, vertice fastigiato*, &c. p. 219.

Stonor House. { f. 22 (Plott, t. 5, f. 4) *Echinites albidocinereus*, &c. p. 221.

{ f. 25 (Plott, t. 5, f. 3) *Echinites velut laminis*, &c. p. 222.

Lower, or hard CHALK.

Ashton-Rowant. f. 28 (Plott, t. 2, f. 11, and t. 7, f. 9) *Echinites præter radios*, &c. p. 224.

Brightwell, S. } f. 29 (Plott, t. 2, f. 14) *Echinites radiorum punctis*, &c. p. 225.
Ewelme, N. }

Pirton. . . . f. 30 (Plott, t. 3, f. 1 and 2) *Echinites punctis prominentibus*, p. 225.

Royston. . . . f. 54, *Pectunculites albidus*, &c. p. 246.

BRICK-EARTH, or micaceous Blue-marl Clay.

Caistor. } f. 32, *Belennites minimus*, &c. p. 227.
Cawkwell. }

Filley-Bridge. . . { f. 9 App. *Solenites, multi longitudine*, &c. p. 22 App.; this is *Perna aviculoides (var. a)*,
Sowerby's Min. Conch. t. 66, f. 3 and 4; and P.M. liii. p. 128.

Londsbrough.
Specton, Cliff. } f. 32, *Belemnites minimus*, &c. p. 227.
Tetford. . . . }

PORTLAND ROCK, or Aylesbury, &c. Limestone.

Black-Hambleton Hill. f. 31, *Belemnites niger*, *maximus*, &c. p. 226.

Brough. . . . f. 37, *Conchites maximus*, *marginato*, &c. p. 233.

f. 3 (Plott, t. 5, f. 10 and 14) *Ammonis cornu*, *spinâ* &c. 207.

f. 4, *Ammonis cornu*, *striis lateralibus* &c. p. 208.

f. 8, *Ammonis cornu*, *læve*, &c. p. 211.

f. 11, *Buccinites magnus*, *ventricosus*, &c. p. 214. This is *Trochus anglicus* β . Sower. Min. [Conch. t. 142.

f. 12, *Buccinites exiguus*, &c. p. 215.

f. 31, *Belemnites niger*, *maximus*, &c. p. 226.

f. 33, *Conchites major*, *rugosus*, &c. p. 229 †.

f. 37, *Conchites maximus*, *marginato*, &c. p. 233.

f. 38, *Conchites rugosus*, &c. p. 234.

f. 44, *Ostracites minor*, *cardine* &c. p. 238.

f. 45 (Plott, t. 4, f. 18, *Gryphites*) *Conchites anomius rugosus*, &c. (*var.* β) p. 238 †.

f. 52 (Plott, t. 4, f. 3 ?) *Pectunculites*, *densissimis*, &c. p. 245.

Byland-Abbey. . . f. 3 (Plott, t. 5, f. 10 and 14) *Ammonis cornu*, *spinâ*, &c. p. 207.

Crake. f. 37, *Conchites maximus*, *marginato*, &c. p. 233.

* In Phil. Mag. vol. xxxix. p. 96 Note, I mentioned the reasons, which (with hesitation) induced me formerly to consider the Bugthorp Shells, as belonging to the *Lias Strata*: unfortunately, in vol. lii. p. 353, I had forgotten these proper doubts on the subject.

† This Shell resembles *Unio crassiusculus* (Sowerb. Min. Conch. t. 135) from the Crag Marl; but its place in the series of *Strata* being different, the differences that exist, should entitle this to a distinct name.

‡ This somewhat resembles *Gryphæa incurva* (Min. Conch. t. 112), but without doubt, I think, its different stratigraphical place, is accompanied by such specific differences, as should entitle it to a peculiar name.

- Elliker.
 Filley-Bridge. } f. 43 (Plott, t. 4, f. 19) *Ostracites maximus*, rugosus &c. (*var. α*) pp. 236, and 21 app.
 { f. 3 (Plott, t. 5, f. 10 and 14) *Ammonis cornu*, spinā, &c. p. 207.
 { f. 14 (Plott, t. 4, f. 1?) *Buccinites lævis*, albidus, &c. (*var. α*) p. 216.
 { f. 21, *Echinites vertice plenior*, &c. p. 221.
 Hinderskelfe. } f. 43 (Plott, t. 4, f. 19) *Ostracites maximus*, rugosus &c. (*var. α*) pp. 236, and 21 app.
 { f. 45 (Plott, t. 4, f. 18 *Gryphites*) *Conchites anomius*, rugosus, &c. (*var. β*) p. 238: See Note †
 { f. 48 (Plott, t. 4, f. 10, 12 and 13?) *Pectinites rarioribus striis*, p. 242. [in last page.
 { f. 37, *Conchites maximus*, margine lato, &c. p. 233.
 { f. 45 (Plott, t. 4, f. 18, *Gryphites*) *Conchites anomius*, rugosus, &c. (*var. β*) p. 238: See
 Lonsborough, W. } Note † in last page.
 { f. 53 (Plott, t. 3, f. 14, 15 and 17?) *Pectunculus cinereus*, &c. p. 245.
 Newton. { f. 14 (Plott, t. 4, f. 1?) *Buccinites lævis*, albidus &c. (*var. α*) p. 216.
 { f. 36, *Conchites albidus*, oblongus, &c. p. 232.
 Nunnington. { f. 1 (Plott, t. 5, f. 15?) *Ammonis cornu maximum*, &c. pp. 205, and 21 app.
 { f. 3 (Plott, t. 5, f. 10 and 14) *Ammonis cornu*, spinā, &c. p. 207.
 Pickering. { f. 14 (Plott, t. 4, f. 1?) *Buccinites lævis*, albidus, &c. (*var. α*) p. 216.
 { f. 48 (Plott, t. 4, f. 10, 12 and 13?) *Pectinites rarioribus striis*, p. 242.
 { f. 4, *Ammonis cornu striis lateralibus*, &c. p. 208.
 Scarborough, SW. { f. 33, *Conchites major*, rugosus, &c. p. 229: See Note † in last page.
 { f. 56 (Plott, t. 3, f. 13 and t. 4, f. 6) *Pectunculus anomius*, &c. (*var. α*) p. 247.
 Specton, shore. { f. 1 (Plott, t. 5, f. 15?) *Ammonis cornu maximum*, &c. pp. 205, and 21 app. [and 23 app.
 { f. 4, *Ammonis cornu striis lateralibus*, &c. p. 208.
 { f. 5, and f. 11 app. (Plott t. 5, f. 12) *Ammonis cornu 5 anfractum*, &c. (*var. β*) pp. 209,
 { f. 57, *Pectunculus anomius*, trilobus, &c. (*var. α*) p. 249.*

* This resembles *Terebratula media* (Sow. Min. Conch. t. 85, f. 5.) but it doubtless is sufficiently different to deserve a distinct name.

Thornton. f. 48 (Plott, t. 4, f. 10, 12 and 13 ?) *Pectinites rarioribus striis*, p. 242.
 Whitwell. { f. 16, *Cochlites lævis*, ore &c. p. 218.
 f. 53 (Plott, t. 3, f. 14, 15 and 17 ?) *Pectunculites cinereus*, &c. p. 245.

CORAL RAG, with *Pisolite* beneath it, sometimes.

Heddington. { f. 13 (Plott, t. 4, f. 2) *Strombites eleganter striatus*, &c. p. 216.
 f. 40 (Plott, t. 7, f. 2) *Bucardites ex albido flavescens*, &c. p. 235.
 f. 51 (Plott, t. 4, f. 11) *Pectinites striis duplicibus*, &c. p. 244.*
 f. 59 (Plott, t. 4, f. 5) *Pectunculites striis densis*, &c. p. 250.
 Shotover-Hill. f. 17 (Plott, t. 6, f. 11) *Cochleomorphites sex spirarum*, (*var. α*) p. 218.

CLUNCH CLAY, with its Clunch or Dogger beds.

Huntingdon. f. 43 (Plott, t. 4, f. 19) *Ostracites maximus*, rugosus, &c. (*var. β*) pp. 236, and 21 app.

ALUM-SHALE, with Cement Balls, Jet, &c.

{ f. 2, *Ammonis cornu spinâ*, &c. p. 206 : This is *Ammonites Walcottii β*, Sow. Min. Conch. t. 106.
 f. 5 and f. 11 App. (Plott, t. 5, f. 12) *Ammonis cornu 5 anfractum*, &c. (*var. α*) pp. 209 and 23 app.
 The largest of these figs. is *Ammonites communis α*, Sow. Min. Conch. t. 107, f. 2 and 3 ; and the smallest fig. is *Ammonites annulatus α*, Sow. Min. Conch. t. 222, f. 2.

Whitby.

{ f. 14 (Plott, t. 4, f. 1 ?) *Buccinites lævis*, &c. (*var. β*) p. 216.
 f. 34, *Conchites sublividus*, &c. p. 230.

CORNBRASH, or Bedford Limestone.

Brise-Norton. } f. 41 (Plott, t. 7, f. 3) *Bucardites costis donatus*, p. 235.

North-Leigh. }

Tees River. }

Wansford. } f. 5 and f. 11 app. (Plott, t. 5, f. 12) *Ammonis cornu 5 anfractum*, &c. (*var. γ*) pp. 209, and 23 app.

* This shell somewhat resembles *Pecten rigidus* (Sow. Min. Conch. t. 205, f. 8) from the Forest Marble : but this shell should be differently named, as belonging to another stratum.

Witney. f. 41 (Plott, t. 7, f. 3) Bucardites costis donatus, p. 235.
UPPER OOLITE, Superior, or Bath Freestone.

Taynton. { f. 17 (Plott, t. 6, f. 11) Cochleomorphites sex spirarum, (var. β) p. 218.
f. 20 (Plott, t. 8, f. 9) Echinites parvulus striis, &c. p. 220.
f. 23 (Plott, t. 5, f. 5) Echinites ovariis, p. 222.
f. 24 (Plott, t. 5, f. 6) Echinites ovariis parvus, p. 222.

FULLER'S EARTH Stratum, or Purple Clay.

Great Rollright. { f. 7 (Plott, t. 5, f. 13) Ammonis cornu striis, &c. p. 211.
f. 58 (Plott, t. 4, f. 4) Pectunculites striis latiusculis, &c. p. 250.

UNDER OOLITE, Inferior.

Burford. f. 27 (Plott, t. 2, f. 9 and 10) Echinites præter quinas strias, &c. p. 224.

Claydon. { f. 6 (Plott, t. 5, f. 11) Ammonis cornu striis, &c. p. 210.
f. 39 (Plott, t. 5, f. 1 &c.) Conchites Mytuloides, p. 235.

Fullbrook. f. 27 (Plott, t. 2, f. 9 and 10) Echinites præter quinas strias, &c. p. 224.
Shutford. f. 42, (Plott, t. 7, f. 4) Bucardites reticulatus, p. 236.

Tangley. f. 27 (Plott, t. 2, f. 9 and 10) Echinites præter quinas strias, &c. p. 224.
BLUE LIAS, or, Water-setting Lime Rock.

Burton on Strather. { f. 45 (Plott, t. 4, f. 8, Gryphites) Conchites anomius, rugosus &c. (var. α) p. 238: This
is Gryphæa incurva α , Sow. Min. Conch. t. 112, f. 1.

IRONSTONE BALLS, Clay Iron nodules in 12th ? Coal Shale of the Derbyshire Coal-measures, Rep. I. p. 161.

Adderton.

Bentley.

Halifax, E.

Leeds, S.

Whitley-Hall.

f. 35 (Muscle) Conchites leviter rugosus, &c. p. 231 : This is Unio subconstrictus, Sow.
Min. Conch. t. 33, f. 1 and 2.

3rd COAL SHALE, Slate-clay, Blae, Plate.

Colne. } f. 10, *Ammonis cornu vix duorum*, &c. p. 213.

Halifax, N.* } f. 49, *Pectinites membranaceus*, &c. p. 243.

Ditto. f. 49, *Pectinites membranaceus*, &c. p. 243.

1st DERBYSHIRE-PEAK LIMESTONE, or Upper Mountain Limestone.

Ashton-Tarne, S. f. 9 (Plott, t. 6, f. 12?) *Ammonis cornu læve*, &c. p. 212.

Broughton. } f. 47, *Conchites anomius, tenuis*, &c. p. 241.

Craven, Mine-field. } f. 9 (Plott, t. 6, f. 12?) *Ammonis cornu læve*, &c. p. 212.

f. 15, *Buccinites lævis, sublividus*, &c. p. 217.

Ditto. f. 50, *Pectinites minor striis capillaribus*, &c. (var. α) p. 243.

f. 55, *Pectunculites subsphæricus*, &c. p. 247.

f. 56 (Plott, t. 3, f. 13, and t. 4, f. 6) *Pectunculites anomius*, &c. (var. β) p. 247.

Frier-head, N. f. 9 (Plott, t. 6, f. 12?) *Ammonis cornu læve*, &c. p. 212

f. 47, *Conchites anomius tenuis*, &c. p. 241

f. 50, *Pectinites minor, striis*, &c. (var. α) p. 243.

f. 56 (Plott, t. 3, f. 13, and t. 4, f. 6) *Pectunculites anomius*, &c. (var. β) p. 247.

4th DERBYSHIRE-PEAK LIMESTONE.

Beresford. f. 50, *Pectinites minor*, &c. (var. β) p. 243.

f. 55, *Pectunculites subsphæricus*, &c. (var. β) p. 247.

f. 56 (Plott, t. 3, f. 13, and t. 4, f. 6) *Pectunculites anomius*, &c. (var. γ) p. 247.

Staffordshire-Moorlands. } f. 56 (Plott, t. 3, f. 13, and t. 4, f. 6) *Pectunculites anomius*, &c. (var. γ) p. 247.

One of the DERBYSHIRE-PEAK LIMESTONES.

Derbyshire Mine-field. f. 57, *Pectunculites anomius trilobus*, &c. (var. β) p. 249.

* At Cathrine Slack, (See Sow. Min. Conch. vol. I. p. 132, and P. M. vol. xlv. p. 218.

An Extract of all such Matters concerning Fossil Shells and Plants, as are mentioned in the Remarks of JOHN STRANGE, Esq. read to the Society of Antiquaries, 28 Jan. 1779, and printed in the "Archæologia." Vol. vi. pp. 35 to 38.

After describing four different *Views*, which are engraven, of the promontory (of whitish Limestone) called *Wormshead*, running out W from the village of Rosilly, in Glamorganshire, Mr. Strange thus proceeds, (in p. 36,) viz. "Wormshead point also merits the attention of naturalists, for the extraneous and marine fossil bodies it contains, especially *Entrochi*, which remaining often prominent above the surface of the Limestone, on account of their resisting better the action of the air, make a singular appearance, and have been supposed to be the hardened excrement of sea Gulls."

"The *phytolypolithi*, or fossil impressions of plants, in the Strata about the coal-mines, are very curious. They are chiefly *Filices*; not of our common indigenous species, but exotics; and I remarked several that seemed to correspond exactly with some of the American *Filices* figured by Plumier in his celebrated *Herbal*.

(p. 37.) "I have since seen much the same impressions in the Strata of the coal-mines of St. Chaumont, in the province of Lionoise, in France: the origin of which, has been so very ably discussed by the late learned naturalist Monsieur de Jussieu (*Mem. de l'Acad.* 1718.) I also observed similar impressions in the coal Strata near Rive de Giez, in the same neighbourhood. Other impressions, nearly of the same kind, are likewise observable in the ironstone of Glamorganshire: particularly between Breton Ferry and Neath: and which appeared to me more curious than any I had ever seen before, or, indeed, since. A recent author, Mr. Beuth (*Julia est montem &c.* 1776, 8vo.) in his account of some extraneous fossil bodies of Lower Germany, has given the descriptions and figures of two curious *phytolypolithi*, greatly resembling some of those, which I remember to have particularly remarked in the said Ironstone. Mr. Beuth may well style these bodies, *rarissimi partus*.

"The hilly promontory a few miles to the west of Cardiff, as well as the blue limestone of the lower country, between Cardiff and Newport, also affords fossil marine bodies in plenty, especially the *Gryphites* oyster, which is not only found abundantly in the lower part of Monmouthshire, and about Purton Passage, but also extends, in considerable aggregates, along the neighbouring midland counties; having myself traced them, either in the *Gravel or Limestone*, through Gloucestershire, Worcestershire, Warwickshire, and Leicestershire; occupying in like manner, the lower parts of those Counties, under the Hills.

"In

“In the high mountains on the confines of Glamorganshire and Brecknockshire, near Yneskedwyn (p. 38) I observed a rock of the same kind of black shelly Marble, that is found in such plenty near Kilkenny in Ireland; and which I afterwards saw in great abundance in Pembrokeshire, where it is also worked. The petrified shells contained in all these Marbles, are striated *conchæ anomieæ*, which are not only exotics, but known to be extremely scarce.”

XXIII. *On the Effect of Vapour on Flame.* By J. F. DANA, Chemical Assistant in Harvard University, and Lecturer on Chemistry and Pharmacy in Dartmouth College.*

To Professor Silliman.

Cambridge, Mass. February 5, 1819.

DEAR SIR, — ABOUT a year since I made some experiments on the effect of steam on ignited bodies, with a view to learn the theory of the action of the “American water-burner.” These experiments were published in an anonymous paper in the North American Review, and have been published in London, without an acknowledgement of their source.

The effect of them concerning bodies is peculiar, and it probably admits of more extensive application to the arts than in the above named instrument alone.

When a jet of steam, issuing from a small aperture, is thrown on burning charcoal, the brightness is increased if the coal be held at the distance of four or five inches from the pipe through which the steam passes; but if the coal be held nearer, it is extinguished, a circular black spot first appears where the steam is thrown on it. The steam in this case does not appear to be decomposed, and the increased brightness of the coal depends probably on a current of atmospheric air, occasioned by the steam. But when a jet of steam, instead of being thrown on a single coal, is made to pass into a charcoal fire, the vividness of the combustion is increased, and the low attenuated flame of coal is enlarged.

When the wick of a common oil lamp is raised, so as to give off large columns of smoke, and a jet of steam is thrown into it, the brightness of the flame is increased, and no smoke is thrown off.

When spirits of turpentine is made to burn on a wick, the light produced is dull and reddish, and a large quantity of thick smoke is given off; but when a jet of steam is thrown into this flame, its brightness is much increased; and when the experiment is carefully performed, the smoke entirely disappears.

* From Silliman's Journal, No. 4.

When the vapour of spirits of turpentine is made to issue from a small orifice, and inflamed, it burns, and throws off large quantities of smoke; but when a jet of steam is made to unite with the vapour, the smoke entirely disappears. When vapour of spirits of turpentine and of water are made to issue together from the same orifice, and inflamed, no smoke appears. Hence its disappearing, in the above experiment, cannot be supposed to depend on a current of atmospheric air.

When a jet of steam is thrown into the flame of a spirit-of-wine lamp, or into flames which evolve no smoke or carbonaceous matter, the same effect is produced as by a current of air.

It appears from these experiments, that in all flames which evolve smoke, steam produces an increased brightness, and a more perfect combustion.

Now, with a very simple apparatus, steam might be introduced into the flames of street-lamps, and that kind of lamp which is used in butchers' shops in London, and in all flames which evolve much smoke. The advantage of such an arrangement would be a more perfect combustion, and a greater quantity of light from the same materials. The flame of the lamps, to which steam is applied, might be made to keep the water boiling which supplies the steam.

I hope the above may not be altogether uninteresting and useless to the readers of your journal.

Very respectfully, your obedient servant,
J. F. DANA.

XXIV. *On the Preservation of Provisions and Goods.* By
JOSEPH MACSWEENEY, M.D.

To Mr. Tillock.

London, Aug. 17, 1819.

SIR, — **T**HE facility with which cutlery of every kind may be put in water deprived of oxygen by iron-filings, and covered with oil, makes it a matter of some consequence to ascertain whether a large quantity of iron has the power of decomposing water at ordinary temperatures. Unbiassed by any opinion on the subject, and desirous only of arriving at truth, I have been led to make the following experiments.

I put a quarter of an ounce of iron-filings in two ounces and a half of water boiled and covered with oil; at the end of twelve days the appearance of the iron-filings was unchanged. I put a large quantity of iron-filings in a phial, and covered them with a layer of warm water about a quarter of an inch thick, the water was covered with oil, a bent tube was attached to the phial, and the end was left under a receiver in a pneumatic apparatus during
a week:

a week: no hydrogen was evolved. A small phial was nearly filled with iron-filings, some boiled water was poured in so as to form a thin layer over them; oil was placed on the water, a bent tube was fitted to the phial, and the end was left under a receiver during a week, as in the former experiment. No trace of hydrogen was discovered. A test tube was nearly filled with mercury, some paper containing iron-filings, moistened with warm water, was forced in a short way; the tube was then filled to the brim with mercury and was inverted over the same fluid. It was kept inverted during five days; no hydrogen was given off. Some paper, containing moistened iron-filings, was forced up to the end of a test-tube with a quill; the tube was then kept inverted over mercury. The mercury rose rapidly in the tube until it attained a certain height, where it remained stationary, evidently from the absorption of the oxygen of the atmospheric air contained in the tube, as was to be expected from the experiments of Dr. Marshall Hall. The glass-vessels containing the water and iron were moved several times during the course of these experiments: perhaps a state of perfect rest may be necessary for the decomposition of water by a large quantity of iron at ordinary temperatures. But it appears difficult to me to reconcile these experiments with the account which states that it takes place rapidly.

In a former paper I stated that I had put some fresh meat in water covered with oil, with some iron to abstract oxygen; and that its appearance was unchanged at the end of seven weeks. In the first place I neglected to state that the meat had been boiled; in the second place I judged of its state of preservation entirely from its appearance through the glass-vessel. After allowing it to remain ten weeks in the water, I removed it: it was found softened, the structure was not much changed, but its odour was offensive. To keep meat as dry and as cool as possible during warm weather is the plan that ought to be attended to.

Where bales of goods are moist by any accident, and it is not in the power of persons to unpack them immediately to dry the goods; it may perhaps be found useful, for preventing mildew, to immerse them in water, and to cover its surface with oil.

I have found that the decay of vegetable substances is very much retarded by immersing them in water covered with oil and deprived of oxygen by iron. A vegetable substance may be sunk by attaching a weight to it; any iron employed for abstracting oxygen ought to be previously removed.

Water in a situation where it is scarce may be preserved, I presume, in an open cask by covering its surface with a thick layer of oil, and by putting in recently prepared charcoal. The trouble of preparing powdered charcoal is an objection to its use.

Large

Large pieces of fresh made charcoal should be attached to a weight, and let down through the oil into the water by means of a cord. The charring of the cask long since recommended, ought to be attended to. After the charcoal has remained for some time in the water, it ought to be drawn up by the cord, and recently prepared pieces should be attached to the weight, and let down. This could be repeated daily, and the water could be drawn from a cock at the lower part of the cask. In this manner charcoal could be put in or removed without exposing the surface of the water to the atmosphere.

I remain your most obedient humble servant,

JOSEPH MACSWEENEY.

XXV. *Occultation of a fixed Star by Jupiter.*

To Mr. Tilloch.

Aug. 24, 1819.

SIR, — ON viewing Jupiter, on the 8th instant, about 9 o'clock in the evening through a small reflecting telescope, I observed a bright star, apparently about the 7th magnitude, nearly in conjunction with him, in a line with his satellites, but on the opposite or western side; all the satellites being at that time on the eastern side of Jupiter. It was at about the same distance from Jupiter's body as the fourth satellite, and of about the same brightness. Having no convenient instrument at hand for measuring the distance, I can only speak from estimation. On the following evening, I could not discover the star; it being either eclipsed by Jupiter's body, or else rendered invisible, through the smoky atmosphere of this place, by the superior brilliancy of Jupiter. On the following evening (the 10th), it was nearly in conjunction with the 4th satellite, and might readily have been mistaken for one of Jupiter's satellites by a casual observer; as the 2d satellite was then eclipsed. On endeavouring to identify this star, which is situated near θ *Capricorni*, and which exceeds in brilliancy many in its immediate vicinity, I was surprised to find that it is not inserted in any catalogue. I have examined in vain the large and small catalogues of Bode, the catalogues of Piazzi, Bessel, Cagnoli, Zach, and the *Histoire Céleste* of Lalande. But in none of them (although many smaller stars are noted) is there the least mention of it. Indeed I was much surprised to find, in the course of this inquiry, how much remained to be done towards forming a complete catalogue of all the stars: but, such an undertaking would be so laborious, that I fear, without the aid of private application, and a more general communication amongst observers, the task will never be fully accomplished.

plished. In taking out a list of the stars between the parallels of $\text{AR } 312^\circ$ and 315° , and the parallels of South Dec. 17° to 19° , I found only *three*, in the large collection of Bode (which contains upwards of seventeen thousand stars), viz. Flamstead's No. 21 and 23, and a small star numbered 101 by Bode. On examining Piazz's catalogue, I find that he has added *four* others, viz. No. 394, 428, 443, and 481 : and from the *Histoire Céleste* (containing the observations of fifty thousand stars) I can deduce only *two* more : thus making the whole number, between the parallels abovementioned, not more than *nine*. Yet there are many others visible through a telescope of very moderate power : and the star which was so nearly (if not quite) in contact with Jupiter, and whose position is *about* $\text{AR } 314^\circ 25'$ and S. D. $18^\circ 9'$, is far superior in brilliancy to any of those abovementioned, excepting No. 21 and 23 ; and must (one would suppose) have been seen by those observers to whose labours and researches we are so greatly indebted for the valuable catalogues which at present exist. However this may be, I hope that what I have thus hastily written may induce others to attend to this subject, and lead to the formation of a new and more extensive *British Catalogue*.

I am, sir, your obedient servant,

ZEUS.

P. S.—Jupiter will be in conjunction again with this star, on the 26th of November ; but his latitude will then be four minutes less than it is at present : so that they cannot then be in contact.

XXVI. Notices respecting New Books.

Memoirs of the Literary and Philosophical Society of Manchester.—Second Series. Vol. III. 8vo. pp. 509.

THE present volume is not inferior in interest and importance to any of its precursors. Some of the papers contained in it are curious ; all of them well worthy of perusal. The following is a list of their titles : Experiments and Observations on Phosphoric Acid, and on the Salts denominated Phosphates. By Mr. John Dalton.—Experiments and Observations on the Combinations of Carbonic Acid and Ammonia. By Mr. John Dalton.—Memoirs of the late Charles White, Esq. F.R.S. with reference to his Professional Life and Writings. By Thomas Henry, F.R.S. &c.—Remarks tending to facilitate the Analysis of Spring and Mineral Waters. By Mr. John Dalton.—Account of the Floating Island in Derwent Lake, Keswick. By Mr. Jonathan Otley.—On the Refractive Powers of Muriatic Acid and Water separate and in a State of Mixture. By Mr. Henry Creighton.—An Essay
on

on the Origin of Alphabetical Characters. By the Rev. William Turner junior, A.M.—Observations on the Rise and Progress of the Cotton Trade in Great Britain, particularly in Lancashire, and the adjoining Counties. By John Kennedy, Esq.—Memoir on a new System of Cog or Toothed Wheels. By James White, Engineer.—On the Flexibility of all Mineral Substances, and the Cause of Creeps and Streets in old Coal-mines. By Mr. John B. Longmire.—Account of the Black Lead Mine in Borrodale, Cumberland. By Mr. Jonathan Otley.—Account of a White Solar Rainbow. By the Rev. R. Smethurst.—Remarks, chiefly Agricultural, made during a short Excursion into Westmoreland and Cumberland in August 1815. By John Moore, jun. Esq.—A Tribute to the Memory of the late President of the Literary and Philosophical Society of Manchester. By William Henry, M.D.—An Essay on the Signs of Ideas; or the Means of conveying to others the Knowledge of our Ideas. By Edward Carbutt, M.D.—Account of some remarkable Facts observed in the Deoxidation of Metals, particularly Silver and Copper. By Sam. Lucas, Esq.—Observations upon the Callous Tumour. By Mr. Kinder Wood.—On the Possibility of reconciling the Scriptural and Profane Accounts of the Assyrian Monarchy. By the Rev. John Kenrick, A.M.—A Descriptive Account of the several Processes which are usually pursued in the Manufacture of the Article known in Commerce by the Name of Tin-plate. By Sam. Parkes, F.L.S.—The Laws of Statical Equilibrium analytically investigated. By Mr. John Gough.—Experiments on the Gas from Coal, chiefly with a view to its practical Application. By William Henry, M.D.—An Enquiry into the Effects produced on Society by the Poor-Laws. By John Kennedy, Esq.—Memoir on Sulphuric Ether. By Mr. John Dalton.—Observations on the Barometer, Thermometer, and Rain at Manchester, from 1794 to 1818 inclusive. By Mr. John Dalton.

Annales Generales des Sciences Physiques, par MM. BORY DE ST. VINCENT, DRAPIEZ & VAN MONS. Tome Premier. Svo. pp. 98. Brussels.

The present is the first number of a new scientific work which has been commenced in the Netherlands by our old and much-esteemed correspondent M. Van Mons, with the assistance of two other gentlemen whose names are not less favourably known to the friends of science. We have derived much pleasure from the perusal of it. The papers are marked by a strong vein of originality, and a spirit of inquiry the enthusiasm of which we are far from being disposed to quarrel with. The following are among the most important:—A Notice on some Improvements on Sir Humphry Davy's Safety-Lamp. By M. Chevrement.—
Vol. 54. No. 256. Aug. 1819. K An

An Account of the Hydraulic Press of Count Real. By M. Van Mons.—On the Mocanere, *Visnea Mocanera*. By M. Bory de St. Vincent.—Description of Eight new Insects. By M. Drapiez, &c. We shall take an early opportunity of enriching our pages with some of the valuable information which they contain.

Works in course of publication by R. Ackermann :

1. *An Historical and Characteristic Tour of the Rhine*, from Mayence to Coblentz and Cologne, in six monthly parts : containing a complete history and picturesque description of a portion of country full of curious and interesting circumstances, as well as resplendent for its landscape, grandeur and beauty. The work will be embellished with twenty-four highly finished and coloured Engravings from drawings expressly made by an eminent artist resident near the banks of the Rhine, and habitually familiar with every part of it. A correct Map of the river, and the territory, according to its last arrangements, through which it flows, is preparing exclusively for this publication.

2. *Letters from Buenos Ayres and Chili*; with an original History of the latter Country: illustrated with Engravings, by the Author of "Letters from Paraguay."

3. *An Elementary Work*, of peculiar interest, on the Construction of the Machines adopted in the Arts and Manufactures; from the French of M. Béfancourt.—It will afford an analytical and perspicuous display of the various combinations which occur in the arrangements of the practical mechanist, with their several applications to use, and constant reference to the engines and machinery of this and other countries. It will be illustrated with thirteen Plates of much novelty and elegance, and be altogether calculated to engage the young student, and gratify the more learned and practical.

XXVII. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES AND BELLES LETTRES OF BRUSSELS.

THE Academy have proposed for competition during the year 1820, the five following questions in the department of science :

First Question.—Suppose a plate of a given figure attached to a surface either by means of screws of a known number, position and force, or by means of some intermediate matter, capable of uniting the one to the other solidly, and the specific tenacity of which is also known ; if to a point of the circumference of this plate an arm be affixed which acts in the same plane with the surface,

surface, it is required to know what resistance this plate will be capable of making against a force applied to this arm as a lever, considering the material, as well of the plate as of the arm and surface, as a perfect mathematical abstraction; that is to say, as perfectly rigid or non-elastic, as infrangible or incapable of breaking, &c.?

Second Question.—A body being suspended from the extremity of a cord, the other extremity of which is fixed to the roof of a room: if this body is made to describe an arc of a certain circle round the fixed extremity; and if, besides, a movement of projection is given to it,—it is required to know the nature of the curve, or rather double curvature, which this body will describe, according to the hypothesis. As is the resistance of the air, so the square of velocity?

Third Question.—If there is an identity between the forces which produce the electrical phenomena, and those which produce the galvanic phenomena,—whence is it that we do not find a perfect accordance between the former and the latter?

Fourth Question.—Many modern authors believe in the identity of the chemical and galvanic forces:—it is required to prove the truth or falsity of this opinion?

Fifth Question.—What is the true chemical composition of sulphurets, as well oxidized as hydrogenized, made according to the different processes; and what are their uses in the arts?

The answer must be supported as far as possible with new facts, and experiments easy of repetition.

XXVIII. *Intelligence and Miscellaneous Articles.*

DISCOVERY OF THE CAUSE OF GRAVITATION.

[From a Correspondent.]

MR. JOHN HERAPATH of Bristol has lately completed the solution of the celebrated problem respecting the cause of gravitation, in which he has been engaged at different times for several years. His researches for the solution of this problem (which was some years ago the object of ardent inquiry by the Royal Society and the continental mathematicians) show that gravitation is only a particular case of a general principle, which comprehends all the great phenomena of nature. It is remarkable that this deduction exactly coincides with the opinion of some of the greatest philosophers of modern times; and, in particular, with that of the late Professor Playfair, in his “*Outlines of Natural Philosophy*.” In the general theorem which Mr. H. has brought out to express the law of gravitation, it is found that the intensity of the attractive force between two ultimate atoms, varies inversely as the

square of the distance affected by a term, which has no influence unless when the atoms are very nearly in contact. This theorem, therefore, not only includes the general law of gravitation, but likewise those of cohesion, affinity, &c. from the application of which to chemical philosophy we may reasonably expect some important discoveries.

APPLICATION OF CHESNUT WOOD TO THE ARTS OF TANNING
AND DYEING.

[From Professor Silliman's Journal.]

Remarks.—A considerable time since, we were confidentially made acquainted with the discovery detailed in the following letter. We have repeated the most important of Mr. Sheldon's experiments, both in relation to tanning and dyeing, and are well satisfied that the discoverer has not overrated, or erroneously estimated, the value of his own results. We are persuaded that the highly *useful* arts alluded to, will derive important aid from the use of a material so abundant and cheap as chesnut wood.

To Professor Silliman.

Springfield, Mass. Feb. 27, 1819.

DEAR SIR,—I send you a more particular account of the newly discovered properties of the chesnut.

This tree (*Fagus Castanea* Linn.) is very abundant in New-England and the middle states; and occurs in the mountainous districts, as far southward as South-Carolina, or perhaps even Georgia. It is one of the stateliest trees of the forest; scarcely less distinguished by the beauty of its foliage, than by the durability of its wood.

By repeated analyses, conducted with the minutest attention to every circumstance which could ensure accuracy, it appears, incredible as it may seem, that the chesnut wood contains twice as much tannin as ross'd * *oak bark*, and six-sevenths as much colouring matter (which gives a black with iron) as logwood. I am aware that nothing could be further from the common apprehension than such results; but the uniform success of a great variety of experiments in tanning and dyeing, in addition to the other kind of evidence, should satisfy the most incredulous.

The leather tanned with it has, in every instance, been superior to that tanned in a comparative experiment with oak bark; being firmer, less porous, and at the same time more pliable. The reason for this difference will probably be found in the *high state of oxygenizement* of the bark, particularly of the epidermis, by which it is rendered to a certain degree acrid and corrosive.

* That is, the inner bark deprived of the epidermis or outer bark by the shaving-knife.

Dr. Bancroft was perhaps the first who noticed the oxygenization of barks. He attributes the dark-brown colour of the epidermis of *his* quercitron to this cause; and as a confirmation of the idea, I have observed that ink made of the epidermis of another kind of bark, though at first not to be distinguished by the colour from that made of the cellular and cortical parts, is incomparably less permanent.

As a material for making ink, the wood of the chesnut is probably unrivalled. Combined with iron in any proportion, it gives, as it is dilute or concentrated, a pure blue or blue-black; while galls, sumach, &c. &c. unless combined with a greater proportion than is consistent with the highest degree of permanency, afford a *black* more or less inclining to a reddish brown. The lake of the chesnut is indeed a blue, and not to be distinguished by the eye from indigo; but when diffused on paper, this same substance becomes an intense shining black. In dyeing, little difference is observable between the chesnut and galls, and sumach, except that the former has a rather greater affinity for wool, &c. than the latter, and of course requires less boiling. Its permanency has been completely tested by long exposure to the sun and the weather; but no doubt can exist on this head, if the position of Berthollet be true, that permanent blacks are formed only by the combination of iron and tannin.

To prepare the chesnut wood for the purposes of tanning, a mode has been devised for reducing it to a suitable degree of fineness. This method consists in the application of knives, either in the direction of, or transversely to, the grain, by a rotatory motion. This mode obviously involves the greatest possible economy of moving power. Messrs B. and M. Stebbins, of West-Springfield, who are making arrangements for going largely into the exportation of the article, have in construction a machine upon this plan.

As might be expected, the inspissated aqueous extract of the chesnut bears a near resemblance in many particulars to catechu. Professor Dewey, of William's College, who at my request has gone through an extensive and elaborate course of experiments, informed me that he obtained a quarter more of the gelatinous precipitate from the former than from the latter. By the taste, the two substances are not to be distinguished, except that the former is more pungent. It leaves upon the tongue the same permanent and refreshing sweetness for which the other is so much prized in the East; where it is used as an article of luxury, with betel nut. Might not the extract be advantageously substituted for catechu, in the celebrated life-preserving composition of Dr. Pearson; the object being to concentrate the

greatest possible quantity of nutritious and tonic substances in the smallest weight?

The colouring properties of the two substances are entirely different. After the discovery, twelve or fifteen years since, of the composition of the *terra japonica*, attempts were made in England to introduce it into the *materia tingentia*, as a substitute for galls; but unfortunately, like the extract of quercitron, it affords with iron nothing but a meagre olive; and Dr. Bancroft states, that in a great number of trials, he was unable, by the greatest accumulation, to produce any thing like a black, even upon wool, much less upon cotton and silk.

A singular fact which I observed in the course of my experiments is worthy of notice. I had prepared for a certain purpose, solutions from the wood of the trunk of a tree about three feet, and from that of a limb about three inches in diameter. The same quantity of wood and of the solvent was employed in both cases. On adding to each the same quantity of the solution of gelatine, abundant precipitates immediately appeared, as usual, apparently much the same in quantity; but to my astonishment, the size of the several congeries in each bore a near proportion to that of the sticks from which they were obtained, not differing much from that of middling and of very small flakes of snow. Is not this an extraordinary fact, evincive of a complication in the arrangement of these bodies hitherto unsuspected? May it not at some future period lead to a *nomenclature of precipitates*; affording, like the crystallography of Haiiy, a new and accurate mode of determining the compositions of substances; and perhaps throwing light upon the obscure subject of chemical, or, if you please, electro-chemical affinities? The *size* of a stick might probably be ascertained with almost as much precision, as by actual admeasurement. The solutions in this experiment were formed by maceration in cold water. When hot water was employed, and the process was completed in two or three hours, the appearance of the precipitate was very different, the congeries being smaller, irregular, and not well defined.

I have only to add, that having taken measures to secure the discovery, both in this country and Europe, it is my wish to bring it into general use as speedily as possible.

I am, Sir, very respectfully, your obedient servant,

WILLIAM SHELDON.

MUD-BATHS.

[From Hallam's Letters from the North of Italy.—Murray.]

The village of Abano is celebrated for its muds, which are taken out of its hot basins and applied either generally or partially
as

as the case of the patient may demand. These are thrown by after having been used, and at the conclusion of the season returned to the hot fountains, where they are left till the ensuing spring, that they may impregnate themselves anew with the mineral virtues which these are supposed to contain. The most obvious of these to an ignorant man are salt and sulphur. The muds are, on being taken out, intensely hot, and must be kneaded and stirred some time before they can be borne. When applied—an operation which very much resembles the taking a stucco cast—they retain their heat without much sensible diminution for three quarters of an hour, having the effect of a slight *rubefacient* on the affected part, and producing a profuse perspiration from the whole body; a disposition which continues more particularly in the part to which they have been applied when unchecked by cold. Hence heat is considered as so essentially seconding their operations, that this watering-place, or rather mudding-place, is usually nearly deserted by the end of August, though there are some who continue to wallow on through the whole of September.

The baths, though sometimes considered as a remedy in themselves, are most generally held to be mere auxiliaries to the muds, and usually but serve as a prologue and interlude to the dirty performance which forms the subject of the preceding paragraph, they being supposed to open the pores and dispose the skin to greater susceptibility.

I can for myself see nothing improbable in the effects which the muds are supposed in many cases to produce; but to pursue a safer mode of reasoning, I have seen myself cases which might alone fairly establish the reputation of Abano. It is true that the muds act very uncertainly; but this is probably the case with every medicament: and I suppose, with the exception of bark and mercury, it may be said there is no such thing as a specific. A gentleman of Feltre, of about two or three and forty, was brought here last year labouring under the effects of a recent paralytic stroke, and contrary to the advice of his physicians, who considered him too much reduced to be able to support the severe discipline of the place. His first attempt confirmed these opinions, and he was obliged through mere debility to suspend his operations; but he was of that class of invalids who determine to get well, and in their own way. Having therefore reposed till he had recovered breath, he returned to the charge, and took the muds and baths for a considerable time, without injury indeed at first, but without any sensible benefit. At length, when all considered his perseverance as fruitless, these began to act; and their effect was as rapid as it had at first been slow. He now mounted on crutches, and after a few days quitted the place,

having arrived at walking with a stick. He returned this spring—completed his cure in three or four weeks—and danced *quadrilles*.

ADDITIONAL NOTICE OF THE TUNGSTEN AND TELLURIUM
mentioned in our June Number.

Part I.—*Description of the Ore.*

Colour, dark brown, almost black, brittle; powder a lighter shade of brown than the mineral, hard, scratches glass, scintillates with steel, with a red spark; a degree of polish produced, where the steel strikes, and when the steel is impressed upon it.

Structure compact, in some places slightly porous; lustre, generally dull, sometimes glimmering, and almost resinous.

Crystals octahedral. Specific gravity of three massive pieces, 5.7, 6, and 6.44; mean, 6.05 nearly; probably that of the crystals would be higher.

Infusible by the blow-pipe even with borax, and does not by strong ignition impart any colour to it or to potash; not magnetic, even in fine powder, not after being heated red hot on charcoal, and in contact with burning grease.

Many specimens decrepitate violently under the blow-pipe. When heated on coals in pieces of considerable size, they often explode with a smart report, and are thrown in fragments sometimes several yards from the fire.

Gangue quartz; accompanying minerals in the same vein, native bismuth, native silver, galena, iron, and copper pyrites, much magnetic pyrites, blende, &c.

Geological Relations.—The country is primitive, and the immediate rock which forms the walls of the vein is said to be gneiss. (We have not seen it.)

Locality; town of Huntington, parish of New Stratford, county of Fairfield, 20 miles west from New-Haven, Connecticut.

Remark. Native bismuth in small quantities has been for several years obtained from this mine, but the shaft has been sunk only about ten feet.

Part II.—*A Variety of the Ore.*

General character as above; but on some parts there is seen a whitish, or yellowish, or sometimes darkish metallic substance; it is in thin plates, like the leaf metals, and sometimes reticulated, and graphic in its disposition; it is soft and easily cut with the knife. In the specimens examined, it was so much blended with the other ore, and so trifling in quantity, that it was not possible to separate it mechanically, so as to examine it separately.

Part III.—*A. Chemical Trials.*

1. Muriatic acid, hot or cold, produces no effect; hot nitromuriatic

muriatic dissolves the ore with energy, red fumes are evolved, and generally a red solution obtained, from which ammonia precipitates red oxide of iron abundantly.

2. A heavy lemon-yellow powder remains, insoluble of course in acids, but easily and completely soluble in warm ammonia.

3. A dark powder, in diminished quantity, again appears, more acid dissolves it in part, and again reveals the yellow powder, which ammonia again dissolves, and so on, till nothing remains but some portion of the gangue.

4. The ammoniacal solution, which contains the oxide of tungsten, is decomposed by acids and by heat, and instantly deposits a white heavy powder, becoming yellowish by standing, and full yellow by heat.

5. This powder is infusible by the blow-pipe; but ignited with borax in a platinum crucible, it became of a superb blue, like smalt, or between that and Prussian blue.

6. The quantity obtained was too small to make it convenient to attempt its reduction to the metallic state; no doubt remained, however, that it was oxide of tungsten, or, as it is sometimes called, tungstic acid.

7. There were traces of manganese; and all the facts perhaps justify the conclusion, that the ore is very similar to the ferruginous tungsten or wolfram.

8. The calcareous tungsten occurs in octohedral crystals; but we have not before heard of this form in the ferruginous species, which generally affects the prismatic forms.

B. REMARK.

We had been for some time inclined to believe that the above ore was ferruginous tungsten; but although fortified by the opinion of Col. Gibbs, we were withheld from announcing it, because the form of the crystals, the specific gravity, the colour, and perhaps some other characters, were not perfectly accordant with European descriptions, and with the specimens in our possession, which are from Saxony and Cornwall.

During the necessary chemical trials (which have, we trust, established the correctness of the above opinion,) we very unexpectedly discovered in some of the ores of tungsten, proofs of the existence of tellurium. The conclusion was induced by the phenomena, for nothing was further from our expectations.

Two fragments were pulverized *by an assistant*, and we therefore cannot say whether they had any external characters different from those of the other pieces; they came, however, from the same part of the vein, and their powder resembled that of the other pieces.

1. Digested in nitro-muriatic acid, a straw-yellow solution, slightly

slightly inclining to green, was obtained, and a black powder was left behind.

2. More acid digested on this powder, gave a deep red solution of iron, and left the yellow oxide of tungsten, which being dissolved in ammonia, the black powder again appeared, and so on, as under 3. Part III.

3. The solution 1, diluted largely with water, deposited an abundant white precipitate, which was very heavy, and rapidly subsided.

4. Alcohol and ammonia respectively produced the same effect, only more decidedly.

5. This precipitate, evidently an oxide of a metal, being collected on a filter and dried, exhibited the following properties.

6. Heated by the blow-pipe on charcoal, it was instantly volatilized in part, and in part decomposed, with an almost explosive effervescence; numerous ignited globules of metal appeared on the charcoal, and burned with an abundant flame of a delicate blue colour, edged occasionally with green.

7. In many trials, these results always occurred, and sometimes a peculiar odour was perceived, at first thought to be owing to arsenic, but it was incomparably feebler, and somewhat resembled that of radishes*.

8. Zinc, iron, and tin, plunged into separate portions of the nitro-muriatic solution, precipitated abundantly a black flocculent substance.

9. On charcoal before the blow-pipe this substance was very combustible, with a blue flame, and was completely dissipated in the form of white oxide, with the above smell.

10. Some of it was obtained on the charcoal in metallic globules; it was a brittle metal, white, with a tinge of red, and foliated, but not so distinctly as bismuth and antimony.

11. The filters on which the white oxide had been deposited, burned almost with explosion, nearly as rapidly as if they had been soaked with nitrate of potash or of ammonia, and the characteristic blue flame appeared while the burning lasted.

12. Other experiments were made upon the metal (not the oxide). It gave to strong sulphuric acid (simply by standing in it in the cold) an amethystine colour, which disappeared as the acid grew weaker, by attracting water from the air.

* This was most remarkably perceived on one occasion, where, under the idea that possibly chrome might exist in the ores, they had been intensely heated in a forge along with pearl-ashes. The mass, when lixiviated, gave only a greenish solution, becoming colourless by nitric acid, and again greenish by an alkali; this was supposed to be owing to iron and manganese. No metal was obtained, except a few minute globules of attractable iron; but the laboratory was filled with white fumes, having the peculiar odour alluded to.

13. With nitric acid it formed a colourless solution, not decomposed by water.

14. It did not dissolve in muriatic acid, till a few drops of nitric acid were added.

15. The white oxide heated with charcoal in a small coated recurved glass tube, afforded brilliant metallic globules, which rose by distillation, collected in the bend of the tube, and resembled drops of quicksilver, except that they were solid.

C. REMARK.

The above facts having induced the conclusion that the metal, thus unexpectedly discovered in the ores of tungsten, was tellurium*, we were led to search for external characters by which to judge what specimens contained it. The ores from Transylvania (the only telluric ores with which we are acquainted) bearing no analogy in appearance or composition to those before us, we were led to inquire whether the tellurium in these latter ores was *in combination* with tungsten, or merely *in mixture*. The external characters detailed in Part II. tend perhaps to fortify the latter opinion. If we mistake not, we there found a proper ore of tellurium mixed with a proper ore of tungsten; but we have also by chemical means found tellurium where similar external characters were not apparent. Before the appearance of our next Number, we hope to obtain pure and better specimens. In the mean time we add the following facts.

1. A crystal, and a massive piece of the kind described under Part II., were digested in nitro-muriatic acid.

2. Both oxide of tungsten and oxide of tellurium were obtained from all of them.

3. Many specimens have been examined which have afforded tungsten only, and no tellurium.

At a convenient time it is hoped that a more complete examination of this subject may be presented to the public.

In the mean time, we may submit to mineralogists and chemists, whether, if this is not a new mineral, it is not at least a new association of two minerals before known. It has not been forgotten that gold and silver are frequently combined with tellurium: neither of them has, however, been discovered (although sought after by proper tests) during the above trials.

Yale College, (U. S.) March, 1819.

* Several of the facts, we are aware, accord with the properties of bismuth, between which and tellurium there are several strong points of resemblance; but a number of other facts appear irreconcilable with the properties of that metal, and of every other except tellurium.

LITHOGRAPHIC PROCESS.

[From Senefelder's Course of Lithography.]

In the chemical or lithographic process of printing, it matters not whether the lines be engraven or elevated; but they must be covered with a liquid to which the printing ink, consisting of a homogeneous substance, will adhere according to chemical affinity and the laws of attraction, while at the same time all those places which are to remain blank, must possess the quality of repelling the printing ink. Now all greasy substances, or such as are easily soluble in oil, will not unite with any watery liquid. That principle is the foundation of this new art. It is not sufficient however to make certain spots of the plate greasy, and others wet; water, generally speaking, not having power sufficient to repel the printing ink from the places on which it ought to be. It is necessary therefore to prepare the surface of the plate, so that in those places which are to remain black, it may reject the printing ink, as if from aversion.

Among the different materials applicable to this new method of printing, the calcareous slate occupies the first place. It possesses not only a strong tendency to combine with unctuous substances, and to retain them obstinately, but it likewise possesses the power of absorbing bodies of a different nature, such as aqueous fluids; and, thus impregnated, will repel oleaginous and unctuous bodies. The best stone of this description is that procured from Solenhofen, in the district of Manheim, three leagues from the town of Neuburg. In general the proper thickness of a plate of it is from two inches to two inches and a half; and, provided their surface be uniform, the harder sorts of stones are the best for all the manners of lithography. They require to be carefully polished: first with pumice-stone and water, until the edge of a ruler applied in all directions every where touches the surface; the stone is then (having been placed horizontally) to be thinly covered with fine sand, mixed with a spoonful of water, to which a little soap may be added; another stone is then to be put on the surface, and moved up and down in different ways; at intervals, fresh sand and water must be applied; and thus two stones are at the same time polished and rendered perfectly level.

The next important requisite for lithography is the chemical ink to draw or write with. The principal qualities of this ink are—its filling the pores of the stone in those places to which it is applied with an oily, greasy substance, and its capacity of resisting the action of aquafortis and other acids. M. Senefelder communicates a variety of recipes for this ink. We transcribe one. Wax (by weight) 2 parts; shellac 4 parts; soap (the common kind, prepared from tallow and soap lees) 4 parts; lamp-black
1 part.

1 part. The manner of making the ink is as follows:—The wax, the shellac, and half the soap are put together in an iron saucepan, and exposed to a strong fire, until the whole mass ignites. When the quantity is reduced by ignition to one half, the saucepan is covered, or put into a pailful of water, to extinguish the flame, and cool the composition. The other half of the soap is then added, keeping the saucepan over a fire, at such a degree of heat as is sufficient for the solution of the soap. The lamp-black, which must be of the finest quality, and must have been previously burned in a close vessel until yellow smoke no longer issued from it, is now added to the composition, stirring it continually all the while. When all has been well mixed, and worked up until it gradually becomes cool, the composition is taken out of the saucepan, when any shape may be given to it. Most of it ought to be formed into small cylinders or sticks; and in that dry state preserved for occasional use; rubbing it down, when wanted, with a few drops of water in a cup, until it is about the thickness of cream.—A softer ink for transferring drawings or writings from paper to the stone is made in the same manner, but with the following ingredients:—Shellac 3 parts; wax 1 part; tallow 6 parts; mastic 5 parts; soap 4 parts; lamp-black 1 part.

[To be continued.]

SKELETON OF A WHALE.

A most interesting point in natural history has occurred in Clackmannanshire.

On Monday last, the 19th instant, while some workmen were employed in making improvements upon the estate of Airthry, the property of Sir Robert Abercromby, bart. about 300 yards south from the east porter's lodge which leads to Airthry castle, they came upon a hard substance, which proved to be that of a large sized whale, dimensions nearly as follow:

	Feet.	Inches.
The head, or crown bone, in breadth	8	5
Ditto, in length	5	0

There are nine vertebræ, some of which are in

diameter, independently of the side processes, 1 8

Breadth, including the processes, ... 3 6

Two bones of the swimming paws:

One of these is in length 5 4

The other (broken) 3 8

Circumference of these bones 3 8

Six broken pieces of bone from one foot in length to 4 0

Thirteen ribs of these:

One is in length 10 0

Ditto in circumference 1 1

And one in length 9 3

Ditto in circumference 1 2

Besides these large bones, a very entire oval and hollow bone was found similar to a shell :	Feet.	Inches.
In length	0	5
In diameter	0	3
Along with the bones, a fragment of the lower part of a stag's horn was also found, measuring in length	1	2
Circumference where a branch had been broken off	0	8

What is most singular regarding this horn is, that at nine inches from the root a hole of about an inch diameter has been perforated, evidently previous to the horn being deposited in the place where it was dug up.

All these bones were found at a depth of from eighteen inches to three feet from the surface of the ground, in what is termed recent alluvial earth, formed by the river Forth, and composed of a blue-coloured sludge or sleek, with a covering of peat earth a few inches thick.

The situation where the bones were dug up naturally refers to a very remote period of time, of which we have no record, when the river Forth was here a great arm of the sea, extending from the Ochill mountains on the north, to the rising ground in the Falkirk district on the south ; and when the very interesting and picturesque greenstone rocks of Abbey Craig, Stirling Castle, and Craigforth formed islands in the midst of deep water.

The skeleton was found lying in a diagonal direction across the line of march betwixt the estates of Airthry and Powis ; and it is very probable that the bones adjoining the tail will be found upon digging into the estate of Powis, the property of Edward Alexander, Esq.

The lovers of natural history are under very great obligations to Sir Robert Abercromby, for the particular care and attention he has paid in preserving these very singular and interesting relics of the animal kingdom.

Sir Robert Abercromby having caused his workmen to proceed in search of the remaining bones, has found no less than thirty additional vertebræ, and one shoulder-bone of a fan shape ; this bone measures in breadth 4 feet ; in length, 3 feet 1 inch.

It is hoped the remaining bones of the skeleton will be found, as Mr. Alexander of Powis has, in the most polite manner, granted permission to cut through the march fence in search of the same.

According to the situation of the Roman stations and causeway at a small distance from whence the skeleton has been found, it may reasonably be concluded that the whale has been stranded at a period prior to the Christian æra.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1819.	Age of the Moon.	Thermo- meter.	Baro- meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
July 15	23	63.	29.80	Cloudy
16	24	68.	29.74	Ditto
17	25	69.	29.70	Fine
18	26	78.	29.50	Ditto
19	27	73.5	29.15	Ditto
20	28	73.5	29.	Ditto
21	29	58.	29.35	Showery
22	new	67.	29.70	Fine
23	1	76.	29.77	Ditto
24	2	76.5	29.76	Ditto
25	3	75.	29.66	Ditto
26	4	73.	29.76	Ditto
27	5	73.	29.78	Ditto
28	6	72.	29.85	Ditto
29	7	73.5	29.63	Ditto
30	8	76.5	29.69	Ditto
31	9	76.	29.57	Ditto
Aug. 1	10	73.5	29.62	Ditto
2	11	68.	29.55	Ditto
3	12	65.5	29.55	Cloudy
4	13	66.	29.55	Ditto
5	full	64.	29.62	Ditto
6	15	68.5	29.55	Ditto
7	16	71.	29.55	Ditto
8	17	70.5	29.70	Ditto
9	18	75.5	29.70	Ditto
10	19	73.	29.61	Fine
11	20	71.5	29.52	Ditto
12	21	78.5	29.87	Ditto—rain in the evening.
13	22	73.	29.40	Rain
14	23	73.5	29.60	Cloudy

METEOROLOGICAL TABLE,
 BY MR. CARY, OF THE STRAND,
 For August 1819.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock Night.			
July 27	60	74	64	30.24	62	Fair
28	60	75	61	.28	47	Fair
29	60	74	66	.20	80	Fair
30	66	78	67	.17	98	Fair
31	68	78	68	.10	70	Fair
Aug. 1	66	78	66	.10	65	Fair with Thunder
2	56	72	57	.02	57	Fair
3	56	69	61	29.99	46	Cloudy
4	64	68	60	30.02	54	Fair
5	66	68	65	.05	40	Cloudy
6	65	73	61	.05	45	Cloudy
7	64	74	60	.10	65	Fair
8	62	70	61	.17	74	Fair
9	65	75	65	.23	55	Fair
10	66	74	59	.20	61	Fair
11	60	71	63	.05	50	Cloudy
12	64	74	66	.01	47	Fair
13	66	72	64	.08	46	Fair
14	64	71	63	.13	55	Fair
15	66	75	70	.20	72	Fair
16	64	76	68	.31	65	Fair
17	66	79	65	.35	66	Fair
18	64	72	64	.38	54	Fair
19	64	73	63	.30	56	Fair
20	60	70	60	.29	57	Fair
21	60	71	62	.32	56	Fair
22	60	71	60	.24	47	Fair
23	66	76	64	.20	65	Fair
24	67	76	66	.07	66	Fair
25	64	76	64	.05	81	Fair
26	66	71	62	.15	62	Fair

N.B. The Barometer's height is taken at one o'clock.

XXIX. *On Mr. TROUGHTON's Expedient for correcting Errors arising from the Excentricity of the Point round which the Indexes revolve in Reflecting Circles.* By Mr. EDWARD RIDDLE, Newcastle-on-Tyne.

To Mr. Tilloch.

SIR, — **W**HILE the success which has attended the researches of the continental mathematicians in every department of physical astronomy is on all hands allowed to be most splendid and imposing; and while the honour is most cheerfully paid which is due to the meritorious individuals who have devoted themselves to such arduous and important investigations; it is satisfactory to reflect, that in practical talent the artists and observers of our own age and country are entirely unequalled and without rivals. What artist in any country, but our own, could be put in competition with Troughton? What observer has raised to himself a name so distinguished as that of Maskelyne, Pond, or Brinkley?

The skill of the distinguished artist whom I have just named, has succeeded in imparting to comparatively small instruments a degree of accuracy which would scarcely seem compatible with their limited dimensions. Many of the expedients by which this has been effected are very admirable. But there is one which has materially contributed to the well merited esteem in which his instruments are held; yet the circumstance is curious, that the correction which it is intended to produce is not, in general, theoretically perfect.

I allude to the expedient which Mr. Troughton has devised to correct, among other errors, that arising from the excentricity of the point round which the indexes may revolve in reflecting circles.

In a paper on these instruments, by Capt. Mendoza Rios, published in the Philosophical Transactions for 1801, is the following remark: "Two noniuses opposite one another might, however, be advantageous in order to correct the errors of excentricity, but in my opinion a greater number ought not on any account to be used."

I know not whether this remark was intended as a hint to Mr. Troughton, who had, for some time previous, made his improved circles with three verniers placed at equal distances from each other on the arc; but the author of the article "Circle," in Rees's *Cyclopædia*, appears to have considered it in that light. He accordingly wrote to Mr. Troughton on the subject, and he has incorporated in the article referred to, an extract from Mr. Troughton's reply. Mr. Troughton observes, that "it is plain that two opposite indices will correct this kind of error perfectly,

and it is equally true, though not so obvious, that three indices will do the same." Meaning, I presume, three placed at equal distances as they are in his circles.

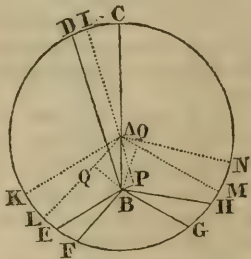
After this and a further extract from the same letter, the author of the article proceeds to observe, that the truth of Mr. Troughton's statement respecting three indexes admits of geometrical demonstration; and he accordingly adds what he appears to consider a satisfactory investigation of the truth of the property.

In Dr. Brewster's *Encyclopædia* also, article "Circle," it is stated that "three readings perfectly correct the error arising from any excentric motion of the index."

But notwithstanding all these authorities, I conceive it will be easy to show that, unless one of the indexes coincide with the line joining the centre and the excentric point round which the indexes revolve, the required compensation will, as I have already said, not be *theoretically* perfect.

With respect to two opposite verniers, it is indeed abundantly obvious that the correction will in all cases be *in theory* complete; as it is a well known property of the circle, that if two straight lines intersect in any point within it, the angle which they make with each other will be measured by half the sum of the intercepted arcs. Hence, while the angle is a constant quantity, the sum of the intercepted arcs is also a constant quantity.

But for three verniers, as they are placed by Mr. Troughton, let A be the centre of the circle; BC, BE, BG, the indexes which revolve about the excentric point B; AC, AK, AM, indexes which revolve in a similar manner round the centre A. Then when one of the indexes revolving round B coincides with one of those revolving round A, as AC and BC do in the figure; it is obvious that KE, GM, which measure the deviations of BE and BG, are equal and of contrary affections, and the correction is therefore complete.



Let now AI, AL, AN, be another position of the indexes revolving round A; and BD, BF, BH, the corresponding positions of those revolving round B. Then if $CD + EF + GH$ is not $= 3 IC$, or if $ID + LF$ is not $= HN$, the correction is imperfect.

On IA produced let fall from B the perpendicular BP, and on AN, AL the perpendiculars BO, BQ, and put $a =$ the angle CAI. Then

Then $\angle BAP = a$, $\angle BAQ = 60^\circ - a$, and $\angle BAO = 60^\circ + a$; also BQ, BP, BO are sines of BAQ, BAP and BAO , to the radius BA . But $\sin 60^\circ - a + \sin a = \sin 60^\circ + a$; wherefore, generally, $BQ + BP = BO$. Now BQ, BP , and BO are sines of LF, DI , and HN , to the radius of the circle. Hence $\sin DI + \sin LF = \sin HN$; and consequently chord $2.DI + \text{chord } 2.LF = \text{chord } 2.HN$. But the sum of the chords of two arcs is greater than the chord of their sum; therefore HN is greater than $DI + LF$, or the correction is theoretically imperfect.

Again: When the sum of the sines of two arcs is equal to the sine of a third arc, the sum of the two arcs is a minimum, and consequently the difference between that sum and the third arc is a maximum, when the two sines or when the two arcs are equal.

Hence the error of the correction is a maximum when $BQ = BP$, or when the $\angle CAI = 30^\circ$, or when one of the indexes revolving round the excentric point is perpendicular to the line drawn through that point and the centre.

Having now, I trust, satisfactorily proved the existence of the error in theory, it is proper that I should add, that as the instrument is actually made, the maximum error is too minute to be computed by common trigonometrical tables. If we suppose an excentricity of $\frac{1}{100}$, the maximum error will be about $26''$, but if we suppose an excentricity of $\frac{1}{1000}$, the maximum error will be a very small fraction of a second.

And when we consider that the actual excentricity is probably, in all cases, a very small part of what we have last supposed, and reflect on the other important purposes to which the contrivance in question is made subservient; we shall still have sufficient reason to admire the ingenuity of its author, though he was wrong in supposing that one of the advantages which he proposed from its adoption would in strict theory be always produced.

But that a mistaken idea should have been generally entertained respecting the theory of an instrument of such importance, is a circumstance which I hope will apologize for my troubling you with this communication, and requesting you to give it a place in the *Philosophical Magazine*.

Your obedient servant,

EDWARD RIDDLE,

Master of the Trinity-House School,
Newcastle-on-Tyne.

Aug. 27, 1819.

XXX. *Experiments on the Gas from Coal, chiefly with a view to its practical Application.* By WILLIAM HENRY, M.D. F.R.S. &c.

[Concluded from p. 126.]

On the Purification of Coal Gas.

THE chief impurities mingled with the gas from coal, which it is desirable and practicable to remove before applying it to use, are carbonic acid and sulphuretted hydrogen gases. The former is of little importance; but the latter imparts to the coal gas when unburned a very offensive smell, resembling that of bilge water, or the washings of a gun-barrel, and the inconvenient property of tarnishing silver plate; and during combustion, gives rise to the same suffocating fumes (sulphurous acid) which are produced by the burning of a brimstone match. The most obvious method of absorbing both the carbonic acid and the sulphuretted hydrogen, is to bring the recent gas into contact with quicklime; and the cheapness of that substance, and facility of applying it, led me, several years ago, to propose it for the purpose*. It has since, I believe, been suggested that the sulphuretted hydrogen may be removed by chlorine; but a sufficient objection to this agent is, that it would also separate the most valuable part of the product, the olefant gas. The transmission of the gas through ignited tubes has also been proposed; but it is a well known property of both the varieties of carburetted hydrogen, that they deposit charcoal when strongly heated; and M. Berthollet has shown that the amount of this effect is proportionate to the increase of temperature†. Some persons practically engaged in lighting with gas have, to my knowledge, been led, by the increase of the quantity of gas which is obtained by passing it through red-hot tubes, to imagine that an advantage is thus gained; and they have not been aware that the gas, when thus treated, sustains a much more than proportional loss of illuminating power.

The quantity of quicklime required for the absorption of a cubic foot of carbonic acid, or of the same volume of sulphuretted hydrogen gas, will be found on calculation not to exceed 1050 grains, or about $2\frac{1}{2}$ ounces avoird. A volume of coal gas containing a cubic foot of each of those impurities will require, therefore, at least 5 ounces of lime applied in the best possible manner. But it is never found in practice that the whole of any gas, when sparingly diffused through another, can be taken out entirely, without using much more of the appropriate agent than,

* Phil. Trans. 1808, page 303.

† *Mémoires de la Soc. d'Arcueil*, iii. 154.

from its known powers of saturation, might have been deemed equivalent to the effect. The proportion employed by Mr. Lee is five pounds of fresh burnt lime to 200 cubic feet of gas. The lime, after the addition of the quantity of water necessary to reduce it into powder, is passed through a sieve, and then mixed with a cubic foot (about $7\frac{1}{2}$ wine gallons) of water. This is found to be enough to purify the gas sufficiently for ordinary purposes; but it still retains a minute proportion of sulphuretted hydrogen, which, from the shade of colour produced in the test, may be estimated at about one ten thousandth of its volume. For some purposes, the same gas is therefore washed a second time with a similar proportion of fresh lime, which, without being removed from the cistern, is again employed to give the first washing to another quantity of fresh gas. After the second purification, the gas produces no change whatever in the test, which preserves its perfect whiteness, thereby demonstrating the complete removal of the sulphuretted hydrogen. In this state of purity, its odour, also, is so much diminished as scarcely to be at all offensive.

In order to ascertain whether any, and what portion of olefiant or carburetted hydrogen gas is lost by the action of the lime liquor, I compared, with the greatest care, the products of the combustion of the recently prepared gas, and of the same gas after one and two washings with lime and water.

	Consumed oxygen.			Gave carb. acid.		
100 measures of the unwashed gas	190	..	..	..	..	108
Gas once washed	..	..	175	..	..	100
Twice washed	..	..	175	..	..	100

The frequent repetition of similar experiments fully satisfied me that the fresh prepared gas from coal does in fact sustain, by agitation with lime liquor, a loss of combustible matter amounting to about 8 or 10 per cent.; but that the second washing is not attended with any further appreciable loss. I found, also, that the recent gas, by being kept a fortnight in bottles completely filled with it, and well stopped so as to exclude all agency of the water in which they were inverted, was diminished in combustible matter about half the foregoing amount. On the other hand, gas which had been washed with lime liquor suffered no change, when kept under like circumstances for an equal time. It is probable, therefore, that what is separated from the unwashed gas, whether by keeping or by the action of lime liquor, is chiefly condensable matter, partly perhaps an ethereal oil and partly a substance which it is desirable to remove, rather than to allow it to be deposited in a solid form, in the small pipes, or in the burners.

The little effect of the lime liquor on the olefant gas, which I had not anticipated, admits however of being satisfactorily explained on known principles. Water and similar fluids absorb, according to Dalton, about $\frac{1}{8}$ th, according to Saussure about $\frac{1}{7}$ th, of their volume of olefant gas. The utmost quantity, therefore, which a cubic foot of lime liquor, acting upon pure olefant gas, could absorb, would be $\frac{1}{7}$ th of a cubic foot. But agreeably to a law discovered by Mr. Dalton, and explained and confirmed by my own experiments*, a cubic foot of lime liquor, when brought into contact with 36 cubic feet of olefant gas mixed with 164 cubic feet of other gases, can absorb only about one-fifth of one-seventh, or $\frac{1}{35}$ th, of a cubic foot of olefant gas. This quantity, which does not exceed $\frac{1}{1260}$ th part of the olefant gas present in 200 cubic feet of the best coal gas, is too trifling a loss to be discoverable by experiment, or to be worthy of being regarded in practice, even when doubled by a second washing. It is therefore consistent with general reasoning, as well as with experiment, that the washing of coal gas with a due proportion of lime liquor should entirely remove the sulphuretted hydrogen gas and other offensive ingredients, without abstracting an appreciable quantity of either of the carburetted hydrogen gases. It is nevertheless important that the quantity of water, employed in washing the gas, should not be increased beyond what is necessary to give the mixture due fluidity, because, under equal circumstances, the power of water to absorb a gas is in direct proportion to the quantity employed.

Such are the principal circumstances that occurred to me as requiring to be investigated, and to be at the same time capable of affording results that may admit of general application wherever coal gas is employed as a source of light. There are others of more limited utility, that may be left to be determined by those persons who are interested respecting them; such as the preference due to different varieties of coal as sources of gas, and sometimes even to other inflammable substances, which, on account of local situation, may be entitled to preference over coal. The facts which have been stated supply, also, data for deciding other questions, which may be suggested by circumstances of partial interest;—for example, whether it may not be advisable, in some cases, to collect only the first portions of gas; or, if all be collected, to reserve different portions apart from each other, and to apply them to appropriate uses. Thus, when coal gas is conveyed in portable gasometers to a distance, (as is now practised by Mr. Lee in supplying his house two miles from the ma-

* Nicholson's Journal, 8vo. vii. 297. and Thomson's Annals, vii. 214.

nufactory*,) it will be important to select that gas, which in a given volume has the highest illuminating power, and which therefore requires vessels of the smallest capacity for its conveyance. Having, I hope, furnished documents for solving questions of this sort, I shall proceed to describe in what manner the facts were ascertained.

Method of Analysis.

1. *Determination of the proportions of carbonic acid and sulphuretted hydrogen gases in coal gas.* In experiments formerly made on this subject, I employed the agency of chlorine to condense both these impurities, and estimated how much of the absorption was due to each, by a rule which I have stated †. Recent experience, however, has led me to distrust this method; and after comparing the effects of several other agents, by experiments on mixtures of known composition, I now prefer the white carbonate of lead, precipitated from acetate of lead by carbonate of ammonia without heat, and therefore fully saturated with carbonic acid. This precipitate it is better not to dry, but, after washing it sufficiently, to leave it under as much water as will give it, when wanted for use, a due degree of fluidity. This mixture may be applied by means of a tube of the capacity of a cubic inch, divided into 100 equal parts, and accurately ground into a short and wider piece of tube, which ought not to contain more than three or four tenths of that quantity. The wider tube being filled with the fluid carbonate of lead, and placed with its mouth upwards under water, the graduated measure full of gas is fitted to it; and the gas and liquid are brought into contact by alternately inverting the two tubes, all violent agitation being carefully avoided. The sulphuretted hydrogen is thus absorbed, and the carbonic acid, being left untouched, is afterwards taken out from the same portion of gas by a similar use of solution of pure potash.

2. *To ascertain the proportion of olefiant gas in the residue left by potash.* From 25 to 30 hundredths of a cubic inch of chlorine gas are passed into a tube of the diameter of about $\frac{3}{10}$ ths of an inch, accurately divided into hundredths of a cubic inch; and the volume of the chlorine is noted when actually in the tube,

* A small carriage upon springs conveys two square close gasometers made of wrought iron plates, and each containing 50 cubic feet of perfectly purified gas, equivalent together to about 6lbs. of tallow. Each gasometer weighs about 160 pounds; and has a valve at the bottom, which is opened by the upright main pipe, the moment the gasometer is immersed in the pit. The strength of one man is found to be sufficient for the labour of removing the gasometer from the carriage to its place.

† Phil. Trans. 1808, page 295.

to avoid errors from its absorption in rising through the water. To this is admitted half a cubic inch (equivalent to 50 measures) of the gas under examination, and the mixture is left, excluded from the direct light of the sun and perfectly quiescent, for fifteen minutes. At the expiration of this time, the remainder is noted, and the diminution which has taken place being divided by 2, the quotient shows the quantity of olefiant gas in fifty measures of the mixture. This process, I am aware, however, does not give results of perfect accuracy; for, in addition to other sources of fallacy, I find that chlorine begins to act on sulphuretted hydrogen much sooner than is generally supposed*, though within the period mentioned, and in such narrow tubes, it does not occasion a sensible diminution of bulk. The method described may, therefore, be considered as affording a tolerably near approximation to the proportion of olefiant gas; and as all the varieties of coal gas were subjected to the test under precisely the same circumstances, the errors must have been of nearly the same amount in all cases, and cannot materially interfere with the fair comparison of the different specimens of coal gas, so far as respects their proportion of olefiant gas.

3. *To ascertain the quantity of combustible matter in gas* which had been deprived only of sulphuretted hydrogen and carbonic acid, a mixture of the gas with a due proportion of oxygen gas was fired by the electric spark over mercury. This method I preferred to slow combustion, carried on with the apparatus which I have described in the Philosophical Transactions for 1808, solely because, when a great number of experiments are necessary, as in this inquiry, the method of detonation is attended with a great saving of time. But on all occasions where only few experiments are required on gases of great combustibility, I prefer slow combustion, both on account of greater safety to the apparatus, and, from the quantities that may be consumed, of greater accuracy also. When rapid combustion is practised, I believe that, on the whole, more accurate results are gained by firing the gas at one operation properly conducted, than at two. The latter method seems to have been preferred by M. Berthollet; but so far as my experience goes, it is more apt to precipitate charcoal from the gas.

To burn each measure of the early and more combustible products of gas, I employed from 3 to 4 measures or upwards of oxygen gas the degree of purity of which had been ascertained. The volume being noted after firing, and again after agitating

* While this sheet was passing through the press, I have noticed a passage in Mr. Brande's Manual of Chemistry (page 156 n.), from which it appears that the speedy action of chlorine on carburetted hydrogen had been observed by Mr. Faraday.

the residue with liquid potash, the last diminution showed the quantity of carbonic acid. The gas left by potash was next analysed by combustion with a due proportion of pure hydrogen*, which showed how much of the residue was oxygen, and how much azotic gas. If more azote was found, than had been introduced as an impurity of the oxygen gas, it was considered as having formed a part of the combustible gas. A single experiment on any kind of gas was never relied upon; and to ensure accurate results, the same gas was fired with different proportions of oxygen. Deducting the pure oxygen found in the residue, from its quantity at the outset, the volume of oxygen gas was learned, which had been spent in saturating a given measure of combustible gas.

In gases free from all admixture with carbonic oxide, it is easy to know how much of the oxygen consumed has been spent in saturating the charcoal; for as oxygen gas by conversion into carbonic acid suffers no change of volume, the quantity which has combined with the charcoal is exactly represented by the volume of carbonic acid produced by the combustion. For example, as 100 measures of olefiant gas afford by detonation 200 of carbonic acid, 200 measures of oxygen must have united with the charcoal of the olefiant gas. But beside these 200 measures, an additional 100 measures of oxygen are found to be consumed, and these must have combined with hydrogen, the other ingredient of the gas, the volume of which in its full state of expansion would be 200 measures, as determined by the fact, that oxygen gas uniformly takes for saturation double its volume of hydrogen gas, and no other proportion.

Nature of the Gas from Coal.

The opinion which I formerly advanced on this subject†, though opposed by writers of so much authority as M. Berthollet and Dr. Murray, still appears to me to be much more probable, than that the varieties of gas from inflammable substances, which may be almost infinitely diversified by modifications of temperature, are, as those philosophers suppose, so many distinct compounds of hydrogen and charcoal, or of hydrogen and charcoal in combustion with oxygen. The reasons that induce me to abide by my original view of the subject are the following:

1. We are acquainted with two distinct and well characterized compounds of hydrogen and charcoal, in one of which a given weight of charcoal is united with a certain quantity of hydrogen, and in the other with double that quantity. Besides these two, no other compound of those two elements has been hitherto proved to exist.

* The method of doing this is given in my *Elements of Chemistry*, vol. i. chap. v. sect. vi.

† Nicholson's *Journal*, 8vo. xi. 68.

2dly. It is inconsistent with experience, that two bodies which, like hydrogen and charcoal, unite by an energetic affinity, should combine in all possible proportions. On the contrary, it is to be expected from analogy in general, and from that of the compounds of charcoal and oxygen in particular, that hydrogen and charcoal unite in few proportions only, and in such a manner that these proportions are multiples or divisors of each other by some entire number.

3dly. All the phenomena may be satisfactorily explained by supposing the gas from coal, and from other inflammable substances, to be mixtures of this kind. For example, referring to the one hour's gas in the first table, we shall find that it contains, in 100 measures, 18 of olefiant gas, which require for combustion 54 measures of oxygen, and afford 36 of carbonic acid. The same gas contains also $77\frac{1}{4}$ measures of another inflammable gas, in the combustion of which $210 - 54 = 156$ measures of oxygen have been spent, and which have afforded $112 - 36 = 76$ measures of carbonic acid. This is as near an approach as can be expected to the properties of carburetted hydrogen, the $77\frac{1}{4}$ measures having consumed very nearly twice their bulk of oxygen, and given an equal volume of carbonic acid. We may, therefore, consider the early products of the gas from cannel as a mixture of about one volume of olefiant gas and four volumes of carburetted hydrogen*.

The early product of gas from Clifton coal does not admit of being thus theoretically resolved into a mixture of olefiant and carburetted hydrogen gases only. For after deducting from the oxygen consumed (164 measures) that spent in saturating the olefiant gas ($10 \times 3 = 30$) we have only 134 measures of oxygen left for the combustion of 90 measures of inflammable gas. These 90 measures, it appears, afford $91 - 20 = 71$ measures of carbonic acid. This portion of the gas does not, therefore, answer to the characters of carburetted hydrogen, since it neither gives an equal volume of carbonic acid, nor consumes a double volume of oxygen. In this case and a variety of similar ones, we can only at present explain the phenomena, by comparing them with hypothetical mixtures of the different known gases. As an example, I shall describe the particulars of the combustion of the first product of Clifton coal, and endeavour to explain the results in the manner which has been suggested.

* I am perfectly aware of the importance of taking the specific gravity of mixed gases, as one datum for determining their proportion in any mixture; but I was prevented from ascertaining it in these experiments by the state of the necessary apparatus, which was found, from long disuse, to have become unfit for the purpose. So far as respects the practical objects of this paper, the omission is of no consequence.

Measures of the gas 11
Mixed with oxygen 39 = 37 pure oxygen + 2 azote.
Total 50
Volume after firing 31
do. after washing by potash 21 = 19 oxygen + 2 azote.
18 oxygen consumed.

In this case, the diminution by firing is 19 measures; that by potash, which denotes the carbonic acid, 10 measures; and the gases consumed are $11 + 18 = 29$. Let us examine what mixture of gases will account for the appearances.

M. of infl. Gas.	Take Oxyg.	Give Carb. Acid.	Dimin. by firing.
1·1 Olefiant ..	3·3 ..	2·2 ..	2·2
7· Carb. hydr. 14·		7· ..	14·
1· Carb. oxide 0·5		0·5 ..	1·
2· Hydrogen ..	1· ..		3·
11·1	18·8	9·7	20·2

The sums of the numbers thus theoretically obtained do not, it is true, exactly correspond with the experimental ones; but they approach as nearly as, from the nature of the subject, can be expected, the greatest disagreement (that in the diminution by firing) not much exceeding $\frac{1}{20}$ th of the observed amount.

In a similar manner we may explain the composition of the lighter and less combustible products obtained at advanced periods of the distillation. For example, a portion of the last product of gas from cannel, distilled in a glass retort, gave the following results :

Measures of gas 20
Mixed with oxygen .. 30 = 28 pure oxygen + 2 azote.
Total .. 50
Fired 22
Washed with potash .. 18 = 14·7 oxygen + 3·3 azote.
13·3 oxygen spent.

In this experiment, 1·3 more azote were found in the residuum than can be traced to the oxygen employed. The combustible gas was, therefore, only $18\frac{2}{3}$ measures; the carbonic acid produced 4; the oxygen spent 13·3; and the diminution by firing 28. The following supposed mixture will explain these facts.

Measures. of	Take Oxyg.	Give Carb. Acid.	Diminished by firing.	In
2 Carb. hydr. ..	4 ..	2 ..	4 ..	
2 Carb. oxide ..	2 ..	2 ..	2 ..	
15 Hydrogen ..	$7\frac{1}{2}$..		$22\frac{1}{2}$..	
19	$13\frac{1}{2}$	4	$28\frac{1}{2}$	

In this instance the hypothetical constitution coincides even more nearly with the facts than in the former case. It must, indeed, be acknowledged that the explanation rests on hypothesis only; but it is on an hypothesis which is perfectly consistent with a copious and increasing induction of facts, all tending to establish a limitation to the proportions in which bodies combine; while the opposite explanation is at variance with this general law of chemical union.

XXXI. *First Report of the Commissioners appointed to consider the Subjects of Weights and Measures.*

May it please Your Royal Highness,

WE, the Commissioners appointed by Your Royal Highness for the purpose of considering how far it may be practicable and advisable to establish within His Majesty's dominions a more uniform system of weights and measures, having obtained such information as we have been able to collect, beg leave to submit with all humility the first results of our deliberations.

1. We have procured, for the better consideration of the subject referred to us, an abstract of all the statutes relating to weights and measures which have been passed in the United Kingdoms from the earliest times; and we have obtained from the County Reports, lately published by the Board of Agriculture, and from various other sources, a large mass of information respecting the present state of the customary measures employed in different parts of the United Kingdom. We have also examined the standard measures of capacity kept in the Exchequer, and we have inquired into the state of the standards at length of the highest authority. Upon a deliberate consideration of the whole of the system at present existing, we are impressed with a sense of the great difficulty of effecting any radical changes, to so considerable an extent as might in some respects be desirable; and we therefore wish to proceed with great caution in the suggestions which we shall venture to propose.

2. With respect to the actual magnitude of the standards of length, it does not appear to us that there can be any sufficient reason for altering those which are at present generally employed. There is no practical advantage in having a quantity commensurable to any original quantity existing, or which may be imagined to exist, in nature, except as affording some little encouragement to its common adoption by neighbouring nations. But it is scarcely possible, that the departure from a standard once universally established in a great country, should not produce
much

much more labour and inconvenience in its internal relations than it could ever be expected to save in the operations of foreign commerce and correspondence, which always are, and always must be, conducted by persons to whom the difficulty of calculation is comparatively inconsiderable, and who are also remunerated for their trouble, either by the profits of their commercial concerns, or by the credit of their scientific acquirements.

3. The subdivisions of weights and measures, at present employed in this country, appear to be far more convenient for practical purposes than the decimal scale, which might perhaps be preferred by some persons for making calculations with quantities already determined. But the power of expressing a third, a fourth, and a sixth of a foot in inches, without a fraction, is a peculiar advantage in the duodecimal scale, and for the operations of weighing and of measuring capacities, the continual division by 2 renders it practicable to make up any given quantity, with the smallest possible number of standard weights or measures, and is far preferable in this respect to any decimal scale. We would therefore recommend, that all the multiples and subdivisions of the standard to be adopted should retain the same relative proportions to each other as are at present in general use.

4. The most authentic standards of length which are now in existence being found upon a minute examination to vary in a very slight degree from each other, although either of them might be preferred without any difference that would become sensible in common cases, we beg leave to recommend, for the legal determination of the standard yard, that which was employed by General Roy in the measurement of a base on Hounslow-heath, as a foundation for the trigonometrical operations that have been carried on by the Ordnance throughout the country, and a duplicate of which will probably be laid down on a standard scale by the Committee of the Royal Society appointed for assisting the Astronomer Royal in the determination of the length of the pendulum; the temperature being supposed to be 62 degrees of Fahrenheit, when the scale is employed.

5. We propose also, upon the authority of the experiments made by the Committee of the Royal Society, that it should be declared, for the purpose of identifying or recovering the length of this standard, in case that it should ever be lost or impaired, that the length of a pendulum vibrating seconds of mean solar time in London, on the level of the sea, and in a vacuum, is 39·1372 inches of this scale; and that the length of the metre employed in France, as the ten-millionth part of the quadrantal arc of the meridian, has been found equal to 39·3694 inches.

6. The definitions of measures of capacity are obviously capable of being immediately deduced from their relations to measures
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of length ; but since the readiest practical method of ascertaining the magnitude of any measure of capacity is to weigh the quantity of water which it is capable of containing, it would, in our opinion, be advisable in this instance to invert the more natural order of proceeding, and to define the measures of capacity, rather from the weight of the water they are capable of containing, than from their solid content in space. It will therefore be convenient to begin with the definition of the standard of weight, by declaring that 19 cubic inches of distilled water, at the temperature of 50 degrees, must weigh exactly 10 ounces of troy, or 4,800 grains ; and that 7,000 such grains make a pound avoirdupois ; supposing, however, the cubic inches to relate to the measure of a portion of brass, adjusted by a standard scale of brass. This definition is deduced from some very accurate experiments of the late Sir George Shuckburgh, on the weights and measures of Great Britain ; but we propose at a future period to repeat such of them as appear to be the most important.

7. The definitions thus established are not calculated to introduce any variation from the existing standards of length and of weight, which may be considered as already sufficiently well ascertained. But, with respect to the measures of capacity, it appears, from the report contained in the Appendix (A), that the legal standards of the highest authority are considerably at variance with each other: the standard gallon, quart, and pint of Queen Elizabeth, which are kept in the Exchequer, having been also apparently employed, almost indiscriminately, for adjusting the measures both of corn and beer ; between which, however, a difference has gradually, and, as it may be supposed, unintentionally, crept into the practice of the Excise ; the ale gallon being understood to contain about $4\frac{1}{2}$ per cent. more than the corn gallon, though we do not find any particular act of parliament in which this excess is expressly recognised. We think it right to propose that these measures should again be reduced to their original equality ; and at the same time, on account of the great convenience which would be derived from the facility of determining a gallon and its parts, by the operation of weighing a certain quantity of water, amounting to an entire number of pounds and ounces without fractions, we venture strongly to recommend, that the standard ale and corn gallon should contain exactly 10 pounds avoirdupois of distilled water, at 62° of Fahrenheit, being nearly equal to 277.2 cubic inches, and agreeing with the standard pint in the Exchequer, which is found to contain exactly 20 ounces of water.

8. We presume that very little inconvenience would be felt by the public, from the introduction of this gallon, in the place of the customary ale gallon of 282 cubic inches, and of the Winchester

chester corn gallon, directed by a statute of King William to contain 269, and by some later statutes estimated at $272\frac{1}{4}$ cubic inches; especially when it is considered that the standards, by which the quart and pint beer measures used in London are habitually adjusted, do not at present differ in a sensible degree from the standard proposed to be rendered general. We apprehend also, that the slight excess of the new bushel above the common corn measure would be of less importance, as the customary measures employed in different parts of Great Britain are almost universally larger than the legal Winchester bushel.

9. Upon the question of the propriety of abolishing altogether the use of the wine gallon, and establishing the new gallon of 10 pounds, as the only standard for all purposes, we have not yet been able to obtain sufficient grounds for coming to a conclusive determination; we can only suggest, that there would be a manifest advantage in the identification of all measures of the same name, provided that the change could be made without practical inconvenience: but how far the inconvenience might be more felt than the advantage, we must leave to the wisdom of His Majesty's Government to decide.

10. In the mean time it may be advisable to take into consideration the present state of the numerous and complicated laws which have been enacted at various times for the regulation of the weights and measures employed in commerce; and the abstract of these laws, which we have prepared, will be found in the Appendix (B) of this Report. We must, however, reserve for a future occasion the information which we have procured respecting the customary weights and measures of the different counties, as we have not yet been able to reduce our abstract into the most convenient form for affording a connected view of this branch of the subject referred to us.

(Signed)	JOS. BANKS.	WM. H. WOLLASTON.
	GEORGE CLERK.	THOMAS YOUNG.
	DAVIES GILBERT.	HENRY KATER.

Soho-square, June 24, 1819.

We think it but just to state, that the main ideas now brought forward by the Commissioners, with a few modifications, were laid before the public more than two years ago, by a gentleman who, though he is not upon this commission, was recommended to be placed upon it by the late Lord Stanhope, at the time when he caused to be thrown out of the House of Lords the absurd bill which had been carried through the House of Commons by some of the gentlemen on the present commission.

In the *British Review* for February 1817, there appeared an interesting and elaborate article on weights and measures, which
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has been generally, and we believe correctly, ascribed to Dr. Olynthus Gregory, of the Royal Military Academy, Woolwich. It was soon afterwards published in a separate pamphlet by Baldwin, Cradock, and Joy. That our readers may judge to what extent the present Commissioners have adopted Dr. Gregory's train of thinking, and how far their slight deviations may be real improvements, we shall lay before them an extract or two from his dissertation :—

“ Let us now endeavour to draw some practical conclusions from this long train of inquiry. With this view we would recommend that the standard foot, to be legalized in future, should agree either with that on Bird's scale made for General Roy (still used in the Trigonometrical Survey) or that on Bird's Parliamentary scale of 1758 (in the custody of the Clerk of the Journals of the House of Commons); either of these being regarded as the 27,404th part of the base on Hounslow-heath, and as equal in length to the prismatic plate that is suspended, and vibrates 36,469 times in five hours, as already described; or rather, that vibrates a certain number of times, agreeably to the result of experiments to be instituted for that purpose under the direction of Parliament. Several rods or plates should be made to agree exactly with this foot, in some fixed temperature,—suppose that of 56 or of 60 degrees Fahrenheit; and several other 2-feet, 3-feet, and 4-feet rods should also be constructed. The length of all may easily be adjusted to the 5000dth part of an inch, by means of a micrometer screw such as is described in Lord Stanhope's account of his monochord; and if the ends of all of them are precious stones set in and ground down to the precise length, the standards will, we apprehend, be rendered more durable than by any other process; except, perhaps, that in which the extremities of the lineal unit shall be shown by metallic points let into the faces of the measures. These, properly stamped, should be lodged at suitable places in the metropolis, and at others in the county towns, as well as other large towns, in the custody of proper officers.

“ Instead of dividing this foot into inches, or twelfth parts (a denominator which always gives a circulating decimal by division, except when the numerator is 3, 6, 9, or their multiples), we would recommend that it be divided into tenths, and each of these again into tenths, or hundredths of a foot. Such division and subdivision would tend to simplify computation, at the same time that they would answer every purpose proposed to be attained by the use of inches and their 12th parts. Together with these we would allow to practical men the binary division of the foot. Although the yard be a multiple of the foot (its triple), which is inconsistent with the general principle of augmentation and diminution by
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Mr Harris's Calorimeter

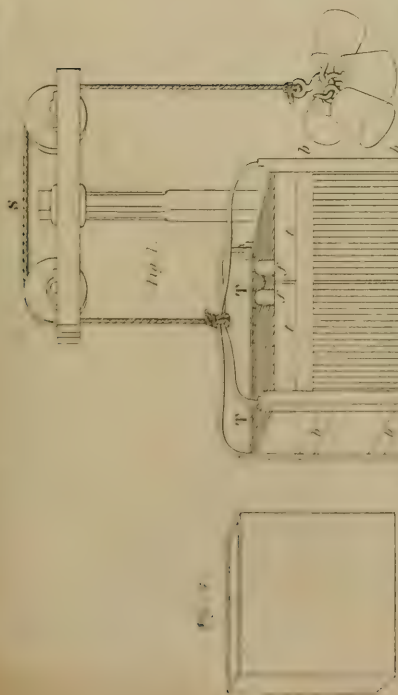


Fig. 3.

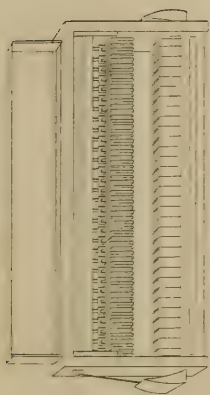
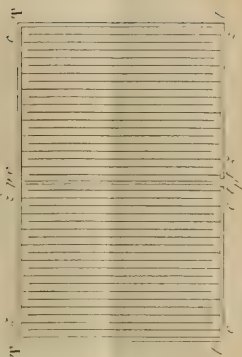


Fig. 2.



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means of the numbers 2 and 5, their powers and products, which we should on the whole recommend; still, as it is one of the oldest of English measures, and is in more frequent use than any other, we conceive it will be advisable to retain it; especially as all the established lineal and square measures above it may remain as they are. The only complex multiple which will then remain in the system will be $16\frac{1}{2}$ feet in a pole; and even that is constituted of $2 \times 2 \times 2 \times 2$ and $1 \div 2$, a power and a submultiple of 2.

“But it may be asked, Why, since we render our standard of length determinable by means of a pendulum, do we limit it to the precise length of the foot now in use? Why do we not take the yard as the unit; or why do we not adopt some unit that has no obvious relation to either the foot or the yard? We reply, Because we can as well employ the pendulum to fix one absolute length as another; because the main object of fresh legislation is, not to make changes, but to prevent changes, except so far as they become necessary to check incessant fluctuation and irregularity; and, more than all, because by selecting the foot, with the absolute value it now possesses, as the unit of lineal measure, we are enabled to connect with it, by the simplest possible relations, measures of capacity and of weight, which shall deviate from those now employed, the latter not at all, the former in such a manner as to furnish a medium between those which exist.

“Take, for example, ten cubic feet as equivalent to the quarter of dry measure. The inches in this, divided by 8, give 2,160 for the cubic inches in the bushel which we would recommend. The capacity of the bushel thus chosen is nearly an arithmetical mean between 2,168, the capacity of the bushel which is the octuple of Queen Elizabeth’s gallon, and 2,150 $2\text{-}5\text{ths}$ that of the Winchester bushel. The difference from either is less than would be occasioned by pouring corn into the measure just over its brim, and at the distance of 4 feet above it. A fourth of this will give 540 cubic inches for the capacity of the peck; and half of this again gives 270 for that of the gallon, differing only the 270th part from Queen Elizabeth’s gallon. Continuing the successive divisions, we have $67\frac{1}{2}$, $33\frac{1}{4}$, and 8, $7\text{-}16\text{ths}$ cubic inches, for the quart, pint, and gill respectively. Neglecting at once the unnecessary distinctions between the ale and wine gallons, and between the ale, beer, and wine hogsheads, and the multiple 63 (*i. e.* 7×9 , numbers both of which give circulates by division) expressing the gallons in a wine hogshead, we would retain the more natural multiples, and call eight gallons a firkin, two firkins a kilderkin, two kilderkins a barrel, and two barrels or 64 gallons a double barrel. It will be observed that by introducing this name, instead of calling 64 gallons a hogshead, we act in conformity with a principle that should always be kept in view

in this inquiry—we mean that of never giving an old name to a new measure, except it differ almost imperceptibly from the old one that bears the same name, or except the new measure be a medium between several old ones of the same name which are to be abandoned: for this variety of things with one name evidently facilitates imposture.

“Whatever be the means of capacity actually adopted, the expediency of having a new series of standards constructed, and deposited at the Exchequer, and other appropriate offices, is evident. But there is room for diversity of sentiment with respect to the shape which shall be given to these standards. We have every reason to believe that vessels in form of parallelipeds are most easily and accurately constructed. Yet we know that where workmen are employed who are acquainted with the most correct processes for boring, hollow metallic cylinders may be constructed with nearly equal accuracy.”

After a few scientific remarks on this subject, which we here omit, the author proceeds thus:—

“The standards of weight, as well as those of capacity, may be drawn by an obvious process from that of length. It has been ascertained by repeated experiments, from the time of Sir Jonas Moore down to the present, that a cubic foot of distilled water, at the temperature of $56\frac{1}{2}^{\circ}$ on Fahrenheit’s thermometer, and under an atmospheric pressure measured by 29.76 inches on the barometer, weighs precisely 1000 ounces avoirdupois. Consequently, a cubic tenth of a foot of the same liquid weighs an ounce, a cube of two-tenths on each side a half-pound, and a paralleliped of the same base and double height (that is, four-tenths) weighs a pound. Let, then, two cubes and a paralleliped of the dimensions just specified be accurately formed of brass or some other metal, and let each of them be carefully weighed both in air and in water, of the above-mentioned temperature, by means of a good hydrostatic balance; the differences of the weight in air and in water in the several cases will be precisely equal to the avoirdupois ounce, half pound, and pound respectively.

“A standard of weight, suppose a pound, may also be found by placing the paralleliped before mentioned in a vessel, and filling the vessel carefully with water at $55\frac{1}{2}^{\circ}$ deg.; then removing the paralleliped, and weighing together the vessel and the water it contains; then filling the vessel again, and re-weighing it with this additional portion of water; the difference between the two weights is manifestly the weight of 16-1,000dths of a foot of water, and this, as before, is equivalent to a pound avoirdupois. This process, however, is not so correct as the preceding, because of the well-known difference which exists between the quantity
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of water or other liquid which a vessel may be made to contain when its brim is thoroughly dry, and that which it will hold without running over, when its edges have been moistened.

“ The system of weights may be carried on to any extent with great accuracy, from those of the ounce, half-pound, and pound, by means of the process invented by Borda, and called by the French *la methode des doubles pesées*, the only method with which we are acquainted that is entirely independent of friction and of inequality in the arms of the balance*. In extending the avoirdupois system, we think the “long hundred” or “hundred weight” of 112 lbs., and its quarter, should be entirely rejected; not merely because the number 7 enters the constituent factors 4, 7, and 4; but because its use is absolutely contrary to law. The common hundred of 4 times 25, but distinguished by some other name, would be far preferable.

“ With the awkward divisions and denominations of the wool-dealers, we know not what to do. Whether they shall be retained or rejected is a question of expediency. Troy-weight must, we apprehend, be allowed to the goldsmiths and jewellers: the ratio (576 to 700) of its pound to the avoirdupois pound is well ascertained. The use of grains, scruples, drachms, &c. must, doubtless, be allowed as heretofore to the apothecaries.

“ We have now, we trust, brought the results of this long, and, in some respects, intricate inquiry, into a shape that is both practical and scientific. No deviation from existing measures is recommended but where there is palpable irregularity or extreme fluctuation; and the several standards of length, capacity, and weight, are mutually connected by principles which, while they are calculated to satisfy the philosopher, are within the comprehension of all men of moderate intellect and information. A system of such gentle but effectual reformation as we have sketched, may, we conjecture, be introduced without affecting the real interests, or violently shocking the prejudices, of any class of men. It remains that we say a little respecting the legislative means by which this may be best ensured.

* In order to ascertain the weight of a body, W , by this method, place it in one of the plates or basins, B , of the balance, and produce an equilibrium by putting certain bodies in the other basin, B' . Begin, for example, by placing heavier substances in B' , such as may be known to approach towards W in weight. Then place successively in the basin B' bullets, smaller shot, minute fragments of sheet copper, then of paper, &c., till the tongue of the balance is brought permanently to the vertical position: this evidently indicates the horizontality of the arms of the balance. Then gently remove the given body W , and substitute for it in the basin B , such weights, shot, fragments of paper, &c. as will together restore the balance to the horizontal position: the aggregate of these will correctly express the weight of the body W , in whose place they have been substituted.

“ Here, of course, it is not our intention to enter into detail ; but merely to indicate the prominent particulars to which attention should be paid in the Act of Parliament. If our general views are correct, we conceive it should be enacted—

“ That a standard of a foot, ascertained and marked at the extremities as before described, be made of platina, or some other suitable metal ; and that others of 2, 3, and 4 feet respectively be made at the same time, each having two series of divisions on different sides ; that is, tenths of the foot on one side, and halves, fourths, and eighths of the foot on the other. That standards of the several measures of capacity from the bushel down to the gill ; and, besides the usual troy and apothecaries’ weights, a series of avoirdupois, that is, of 100 pounds, of 25 pounds, of 8 pounds, of 1 pound, $\frac{1}{2}$ pound, $\frac{1}{4}$ pound, 1 ounce, $\frac{1}{2}$ ounce, $\frac{1}{4}$ ounce, and of 1 drachm, be also constructed. That five sets of complete and accurate copies of all these, whether of length, capacity, or weight, be likewise made : and that, each series being stamped with appropriate and distinguishing marks, there be deposited, one complete series at the Exchequer, another at the proper office in Westminster, another at Guildhall, another at Founders’-hall, another at the British Museum, and another in the rooms of the Royal Society.

“ That copies of the standards of length, and of the Act of Parliament, be sent to all foreign Governments with which England is at peace.

“ That other exact copies of the several standards be sent to the several county-towns, cities, and boroughs in the realm, having, besides the Exchequer stamps, the name or arms of each city, &c., to which they are sent, marked upon them ; and that they be safely lodged in the custody of some proper officer, not for the purpose of sizing and adjusting new measures (except as below), but for that of detecting error or fraud with regard to measures in use. And that these copies shall be only producible for certain purposes specified in the Act.

“ That another series of copies of the standards, appropriately marked, but in some way distinct from each of the others, be sent to every excise division, and placed in the custody of the supervisor of excise for the time being.

“ That all the measures of the several series be sized and adjusted in a room where they have previously remained at least 24 hours, in a certain assigned temperature, or within certain narrow limits.

“ That all the standards of capacity have marked upon them in permanent legible characters their respective proportions to old measures bearing the same name. On the *gallon* measure, for example, there might be placed—

The 271-282d part of the customary ale gallon.	}	Standard gallon 271 cubic inches, or 0·1538287 of the cubic foot.	}	and 1,40-231st of the customary wine gallon.
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“ That certain officers be appointed, one of them at least being competently acquainted with mechanical and mathematical science, under the direction of the Lords Commissioners of His Majesty’s Treasury, to take care of the standards deposited in the Exchequer, and to ascertain, size, and mark, within the prescribed limits of temperature, all new measures of weight and capacity, which are in future to be employed out of the limits of the jurisdiction of the cities of London and Westminster, and of the Founders’-hall charter. That since some irregularity will inevitably attend the sizing and stamping of weights and measures of capacity in county towns, all such, intended for use in the country, wherever made, shall be tried and marked at the above-mentioned office : and that the expenses of such office be, at least in part, provided for by certain small sums payable upon the assizing of measures and weights, and by the amount of licenses to be granted to the makers of such measures and weights.

“ That it be allowable for yard-wands and carpenters’ two feet rules, to be tried and adjusted by the measures deposited in the county towns and boroughs, agreeably to methods prescribed by the Act.

“ That after the new standards are constructed, all the old weights and measures at the Exchequer shall be deposited in a separate apartment, safely locked up, and never in future be taken out but in the presence of some high officer of His Majesty’s Government, or by his express direction for some specified object. And that it be recommended that the measures now deposited at Guildhall, &c. be also locked up, to make way for the new standards.

“ That, in future, all new weights and measures be marked with the name and abode of the maker, the initials of the examiner at the proper office, and the temperature in which they were adjusted.

“ That all heaped measures be abolished, and, if it be found necessary, new measures be substituted instead of them.

“ That the use of old and of new measures (where they differ) be optional for twelve months, or some other fixed time, after the passing of the Act. That from the end of that period all pleadings in actions and suits must express the size, quantity, or value of things, in terms conformable to the new measures.

“ That after two years it shall be penal to charge a weight or measure for any commodity, which does not agree with the standards

dards appointed by the Act; and after four years it shall be penal to employ in trade or commerce any measure or weight which is not marked agreeably to the directions of the Act.

“That it be highly penal for any person to make or sell measures and weights, but in compliance with the requisitions of the Act.

“That tables of equalization and reduction, of inches into tenths of feet, ale and wine gallons into standard gallons, &c. be computed, and attached to the Act, as well as printed for separate circulation: and that country schoolmasters, who teach arithmetic, be entitled to receive a copy of the Act of Parliament and of these tables, from a prescribed parish officer—first gratuitously, and afterwards at a fixed price.

“That suitable officers under the sheriffs of counties, mayors of corporations, &c., be appointed to read the Act at all fairs, and once a month at markets, for five or seven years.

“That all former Acts, or clauses of former Acts, that are either contradictory to one another or to this Act, be repealed.

“That the requisite exemptions be made in favour of goldsmiths and apothecaries; and in reference to the privileges of the cities of London and Westminster, the Founders' Company, &c.

“That the effects of the proposed changes (slight as they are) upon the excise, taxes, rates, allowances, parochial customs, and different branches of trade and commerce, be ascertained; and the manner of providing for them be carefully explained in the Act.”

XXXII. *Observations on the Relation of the Law of Definite Proportions in Chemical Combination, to the Constitution of the Acids, Alkalis, and Earths.* By JOHN MURRAY, M.D. Fellow of the Royal College of Physicians, of the Royal Society of Edinburgh, the Geological Society of London, &c.; Lecturer on Chemistry, and Materia Medica and Pharmacy.

[Concluded from p. 100.]

THE compounds of nitrogen with oxygen present considerable difficulties; some of them are not easily obtained insulated; the specific distinctions, therefore, which constitute the series, have been variously represented, and the subject is still imperfectly elucidated. Two of them, however, are determined with sufficient precision, from which we may proceed—those constituting the two oxides, the first, nitrous oxide, being composed of 10 of nitrogen with 5.7 of oxygen; the second, nitric oxide, of 10 of nitrogen with 11.4 of oxygen.

These combinations are conformable to the usual law of definite
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nite proportions, the oxygen in the one being to that in the other as 2 to 1. It might be expected, therefore, that in the two succeeding compounds admitted by chemists, nitrous and nitric acids, the same ratio would be observed; that the oxygen in the one would be as 3, and in the other as 4. It appears, however, from experimental evidence, that these are not the proportions.

Nitric acid, the extreme of the series, is the one most capable of being obtained uniform, and the composition of which admits, therefore, of the most exact determination. Even with regard to it there are discordant results; but from those of greatest accuracy the proportions may be fixed at 10 of nitrogen with 28·5 of oxygen,—a proportion of oxygen which is to the first not the multiple of 4, but 5, and which therefore breaks the uniformity of the series.

The composition thus assigned, however, is that of what is called real nitric acid, free from the portion of combined water supposed to exist in the acid in its insulated state, and abstracted when it passes into its saline combinations. If we exclude this hypothesis, and consider this water as existing in the acid in the state of its elements, and the acid, therefore, as a ternary compound of nitrogen, oxygen, and hydrogen, this portion of oxygen is of course to be admitted into the calculation. But still this does not obviate the difficulty. The quantity of this water has been variously estimated. If the estimate by Dr. Wollaston be admitted, that of 0·25, it gives the proportion of 10 of nitrogen and 40 of oxygen, which makes the multiple of oxygen 7, a result equally distant from the regular progression.

The composition of the intermediate compound, nitrous acid, it has been found still more difficult to determine, principally from the difficulty of obtaining it insulated, and free from all intermixture of nitric acid and nitric oxide. Different views have been proposed with regard to it to remove the difficulty. Gay-Lussac, in particular, assumed the existence of two compounds, pernitrous and nitrous acid, intermediate between nitric oxide and nitric acid, which, from their proportions, afforded the intermediate multiples 3, 4, that of real nitric acid being considered as 5. But Dulong has shown that these acids are the same. He has also obtained nitrous acid in its insulated state; its composition is 10 of nitrogen with 22·8 of oxygen; a proportion of oxygen which gives the multiple 4, so that the series is still incomplete, being that of 1, 2, 4, and either 5, or 7.

When this acid is acted on by an alkaline base it is decomposed, one part passes to the state of nitric acid, and forms a nitrate, and the other forms a nitrite. It might be supposed, therefore, that one portion of it yields oxygen to the other, and that

thus a subnitrous acid is formed, which might afford the intermediate proportion. Nitric oxide gas, however, is disengaged, so that there is probably no reduction in the degree of oxygenation. And if there were, it would, conformably to the principle illustrated under the consideration of sulphuric acid, be replaced by the oxygen of the base, and form the ternary compound constituting the nitrite, so that the relation of this element to the nitrogen would be the same. There is therefore no evidence of the existence of any definite compound intermediate between nitrous acid and nitric oxide, and the ratio of oxygen in nitrous oxide and these two compounds is that of 1, 2, 4.

The proportion in nitric acid, it has been stated, is that which gives the multiple 5 of oxygen. But this applies to what is called the real acid free from water, and no such compound exists, not even in combination with a base; for, as has been already shown, when an acid yields water from the action of a base, though there is thus an abstraction of a portion of its oxygen, it receives that of the base, and forms a ternary combination, in which the proportion of oxygen to the radical remains the same.

The real composition, therefore, must be determined in its state of hydro-nitric acid. The quantity of combined water, according to the common expression of the fact, existing in it, has been variously stated; but if the estimate in Dr. Wollaston's scale of 0.25 in acid of the specific gravity 1.50 be taken, this gives as the composition 10 of nitrogen, with 40 of oxygen and 1.55 of hydrogen: and this again gives 7 as the multiple of oxygen in the series of compounds,—a result which it is scarcely possible to connect according to the established law with the multiple 4, in the lower compound, nitrous acid.

It is certain, however, independent of this circumstance, that the quantity of water, (or of oxygen and hydrogen equivalent to it,) thus assigned, is not the just proportion, essential to the constitution of the acid; for the specific gravity 1.50 is not the highest at which it can be procured. It is obtained with certainty at 1.55 at 60°, by some chemists it is stated at 1.58, and by Proust even at 1.62. At 1.50, therefore, it must be diluted with a certain portion in addition to the real combined water of the common hypothesis. Dr. Wollaston has observed, that to decompose nitrate of potash so as to afford nitric acid, it is necessary to employ as much sulphuric acid as forms bi-sulphate of potash, and hence each portion of potash from which dry nitric acid is separated, will displace the water from two equivalents of sulphuric acid. One of these portions of water, it may be presumed then, will go as essential to the constitution of the nitric acid, or rather its oxygen and hydrogen will do so; the other is
adventitious,

adventitious, though from the volatility and facility of decomposition of the acid it may not be easily abstracted.

On this view, the composition of the acid will be found to be 100 of nitrogen, 34 of oxygen, and 0.76 of hydrogen, which gives 6 as the multiple of oxygen to the first proportion of that element. The proportion of hydrogen is to the nitrogen as the first or lowest equivalent, that in ammonia being the third, the former being 0.76 to 10, the latter to the same quantity of nitrogen 2.3.

The same view of the composition of hydro-nitric acid may be inferred from the proportion of oxygen and nitrogen in the dry nitrates. In these, as in other analogous cases, the abstraction of oxygen in the formation of water at their formation is compensated by the oxygen of the base; the metallic radical of the latter merely replaces the hydrogen of the acid, and the proportion of oxygen to the radical of the acid remains the same.

It thus appears, that the series of the nitrous compounds is nitrous oxide, nitric oxide, nitrous acid, and nitric acid. The oxygen in the first is to the nitrogen as 5.7 to 10; and taking this first proportion of oxygen as 1, that in nitric oxide is 2, in nitrous acid 4, and in hydro-nitric acid 6,—a ratio sufficiently conformable to the law of definite proportions.

If it were admitted, that the oxygen and nitrogen remaining after the action of hydro-nitric acid, and anhydrous nitrous acid, formed binary compounds which entered into direct combination with the alkali, then from the abstraction of one proportion of oxygen in the one by the formation of water, and in the other by the production of nitric acid, compounds would be formed, intermediate in the former between hydro-nitric and nitrous acid, and in the latter between nitrous acid and nitric oxide, and thus the series of the proportions of oxygen of 1, 2, 3, 4, 5, 6, would be completed. This view, however, is not probable. At the same time, the relation of these elements in these intermediate proportions may exist in other ternary compounds, though they are not found in binary combination, or in the ternary combinations which they form with hydrogen, or with metallic bases.

The composition of the acids, of which phosphorus is the base, is so imperfectly determined, and the most recent experimental researches are so much at variance in their results, that scarcely any satisfactory application of a principle can be applied to them. There is some reason to believe, that the three acids which appear to be of definite composition, the hypo-phosphorous, phosphorous, and phosphoric acid, contain oxygen in proportions affording the multiples 1, 2, 4. The intermediate multiple of 3 is probably to be found in the combination which is established of phosphorous acid acting on a base, conformable to the view illustrated in the analogous case of sulphurous acid,—the acid
receiving

receiving the oxygen of the base, and a ternary compound being formed, in which the whole oxygen and the radical of the base observe the due relation to the radical of the acid. And from the quantity of base which phosphorous acid must saturate, this additional proportion of oxygen will be precisely a multiple of that with which phosphorus combines. Phosphoric acid appears to be formed in the combustion of phosphorus in oxygen, and must therefore be admitted to exist as an insulated binary compound. It is further capable, however, of combining, according to the common expression of the result, with a definite proportion of water, that is, with an additional proportion of oxygen, and with hydrogen equivalent to that proportion. The quantity of this has been variously estimated, and does not appear to be very accurately determined; but it will probably be equal to an additional multiple of oxygen, that is about 14 in 100, and then the series of phosphoric compounds will contain oxygen in the ratio of 1, 2, 3, 4, 5. If the estimate, however, by Berthollet and Berthier were correct, which makes the quantity of combined water equal to 25 in 100, it would be equal to 2 multiples; and the series might be 1, 2, 3, 4, 6. And if phosphorous acid does not combine directly with the elements of the alkaline bases, but forms, as has been affirmed, partly phosphates, partly phosphites, the series will be that of 1, 2, 4, 6, similar to that of the nitrous compounds.

In the muriatic compounds, no regular progression has been discovered, considering either muriatic acid, or chlorine, as the first of the series. Some such progression may perhaps, however, be traced.

Considering muriatic acid as a compound of a radical with oxygen, Berzelius has inferred, from the application of the principle, that the quantity of oxygen in an acid is either equal to, or a simple multiple of the quantity of oxygen in a base which it saturates, that it consists of 41.632 of radical, and 58.368 of oxygen. This applies, however, to what is called the real acid free from water, a compound, the existence of which is not proved. Taking the proportion of water, or rather of its elements in hydro-muriatic acid into calculation, it gives as the composition 31.224 of radical, 65.851 of oxygen, and 2.925 of hydrogen. The proportion of oxygen to the radical in oxymuriatic acid is the same, the only difference between the two being the presence of hydrogen in muriatic acid; in oxymuriatic acid, therefore, the proportion is 32.164 of radical, with 67.836 of oxygen. The next compound is euchlorine, composed of 100 of oxymuriatic acid, with 22.26 of oxygen; this is almost exactly the third of the former; the relation is, therefore, that of 3, 4. Another gas, which has since been discovered by Sir H. Davy, contains a
much

much higher proportion of oxygen, being composed of 100 of oxymuriatic acid with 89; this is exactly 4 multiples, and gives therefore the series of 3, 4, 7. Hyper-oxymuriatic or chloric acid is composed of 100 of oxymuriatic acid with 111 of oxygen, which is another multiple, or 8. It cannot, however, exist insulated, as Gay-Lussac states, without the presence of water; that is, it is a ternary compound, containing probably an additional multiple of oxygen, and thus affording the series of 3, 4, 7, 9. If an error of experiment were supposed with regard to the second, or euchlorine, so as to have deviated from the multiple 5, this would afford a series somewhat regular. But even without assuming this, it is of importance to find in all these, that the proportions are simple multiples of a first quantity. And as the relations of carbon to oxygen and hydrogen, in the composition of the vegetable acids, show the numerous definite proportions in simple multiples in which they combine, so combinations not more numerous may supply the intermediate multiples in the muriatic compounds.

There is a peculiarity in the muriatic compared with the sulphuric and nitric compounds. In the latter, there does not exist any binary compound of the radical with oxygen, in which the proportion of the one to the other is the same as the proportion in which they exist in the ternary compound which they form with hydrogen. There is therefore no oxysulphuric or oxynitric acid. In hydromuriatic and oxymuriatic acids, the proportion of oxygen to the radical is the same, and there is only in the former an addition of hydrogen. Hence the apparent peculiarity of oxymuriatic acid having an excess of oxygen, and the circumstance, that by an addition of hydrogen it is converted into muriatic acid. This, however, is not absolutely peculiar to it, and presents therefore no anomaly. The same thing holds in the relation of carbonic and oxalic acids. In both, the same proportion of oxygen to carbon exists; the oxalic acid only containing, like the muriatic acid, an addition of hydrogen. Did hydrogen act with the same facility on carbonic acid that it does on oxymuriatic acid, it would convert it into oxalic acid in the same manner that it converts the other into muriatic acid. And were the attraction of carbon to metals and inflammables more powerful than it is, so as to bring it into ternary combination with them with oxygen, or its affinity to hydrogen equally strong with that of the radical of muriatic acid, its action, in apparently imparting oxygen, would probably be equally energetic as that of oxymuriatic acid.

The constitution of the alkalis and earths, which I have considered as ternary compounds of radicals with oxygen and hydrogen,

gen, will be found to exhibit, in conformity to this view, a perfect coincidence with the law of proportions. One or two examples will be sufficient for illustration.

Potassium, in the proportion of 100 with 20.5 of oxygen, constitutes the binary compound denominated dry potash, and which is probably the first degree of oxidation. If, in the ternary compound, which constitutes the alkali in its common state, fused potash as it is named, the additional portion of oxygen is equal to this, or the whole quantity is twice that in the first, conformable to the usual law of proportions, then the quantity of water which will be obtained from the subversion of its composition, and which, according to the common doctrine, is water combined with the alkali, will be 16 from 100 of the fused potash. Now, Berthollet assigned the quantity from experiment at 14, and Sir H. Davy at from 17 to 19. The mean of these may be taken at 16, conformable, therefore, to the theoretical application. And this quantity is stated on the authority of Berzelius as the precise proportion. This second proportion of oxygen seems to be established as an insulated binary compound in combination with the radical, as well as in the ternary combination into which hydrogen also enters, if it is perfectly just, what has been asserted, that the excess of oxygen in the product of the combustion of potassium in oxygen is expelled by heat. And if this compound were capable of being acted on by hydrogen, (which it can scarcely be doubted it is,) it would afford another perfect analogy to oxymuriatic acid, as by this action it would be converted into potash, precisely as oxymuriatic acid is by the same action converted into muriatic acid. The facility with which hydrogen is admitted into the binary compound, so as to form the ternary combination, is still greater than the facility with which a similar change is produced in oxymuriatic acid, the addition of water alone producing the effect, converting the peroxide of potassium into potash, and liberating of course the corresponding excess of oxygen.

Sodium combines with a larger quantity of oxygen than potassium does; and therefore soda ought (adopting the language of the common doctrine) to contain a larger quantity of combined water,—the water being always proportional to a multiple of the oxygen combined with the radical. The fact is accordingly conformable to this, 100 of fused soda affording about 24 of water.

Barium, on the contrary, combines with less oxygen. Sir H. Davy, from indirect results, infers, that 89.7 of barium combine with 10.3 of oxygen. In conformity to the law, therefore, barytes ought to afford less water, which is accordingly the case,

100 of hydrate of barytes, as it is named, affording, according to the estimate of Berthollet, 9 of water, according to that of Berzelius about 10·5.

The neutralization of acids and of oxides, by their mutual action, I have already stated, is probably not merely the result of combination, but of subversion of composition. The radical of the acid, and the radical of the base, are in combination with the oxygen which remains after the abstraction of any portion of this element by the formation of water. And the proportions established will be found directly conformable to the relations of these elements. It has been already shown, (page 186,) that the relation of the oxygen in the ternary combination is that which it separately observes to the radical of the acid, and the relation of the radical of the alkaline base is that which it also separately observes to the radical of the acid. And the three elements exist in simultaneous combination. So far the constitution is analogous to the composition of the ternary acids and bases, with this difference, that in these the oxygen and hydrogen are in their respective proportions to the radical of the acid or base, and in the salts the oxygen and the radical of the base are in their due proportions to the radical of the acid. In the conversion of the one into the other, there is merely the substitution of the radical of the base for the hydrogen of the acid, and the abstraction of that portion of oxygen with which the former was combined, and the formation of a portion of water equivalent to this. In the formation of a neutral salt from the union of a binary acid, there is simply the production of a ternary combination, in which the proportion of oxygen to the radical of the acid is increased by that of the base. And the difference in the salts formed by the binary and ternary acids of the same radical, is in the quantity of oxygen being a higher multiple in those of the latter than in those of the former; so that the addition or abstraction of that portion of oxygen converts the one into the other.

There is every reason to infer, that in the ternary acids, and the ternary alkaline bases, while the due relation of oxygen to the radical and of hydrogen to the radical exists, there will be a similar relation in the hydrogen and oxygen to each other. These two elements combine only in the proportion of 1 to 7·5. But there may be other proportions multiples or submultiples of these, in which they exert mutual actions, though they do not in conformity to them form binary combinations, and they may exist under the influence of such actions in ternary combinations. In hydro-sulphuric acid the quantity of oxygen in relation to the hydrogen present is four times the quantity of oxygen which constitutes the composition of water. And this may be a relation actually existing, independent of the others; that is, while the oxygen

oxygen in the proportion in which it is present, acts on the sulphur, and the hydrogen acts on the sulphur, the oxygen and hydrogen likewise act on each other; and these actions are in equilibrium consisting the sulphuric acid. And in all these ternary compounds at least, the elements may exist in these uniform relations, instead of any of them being in any case in intermediate proportions. In like manner, in the compound salts, the two radicals, that of the acid and that of the base, will observe their due relation in proportions to each other.

In the neutral salts, then, there exists neither acid nor alkali; and their decomposition is merely the transfer of the radical of the base in the one to the radical of the acid of the other. The decomposition, for example, of nitrate of barytes by sulphate of potash, consists in the transfer of barium to sulphur and oxygen, and of potassium to nitrogen and oxygen. The quantities must be equivalent to each other; and hence the law of Richter, that the state of neutralization remains*.

In the mutual action of acids and salifiable bases with regard to saturation, the simple rule will be, that in all cases an acid will saturate that quantity of a base, the radical of which is in the equivalent weight to the radical of the acid. And the quantity of oxygen in the salt will be that which constitutes the usual proportion of that element to the radical of the acid.

The capacity of saturation in the different acids and bases, in their reciprocal action, has been proposed as a measure of the force of affinity which they exert, those acids being inferred to have the strongest attractions to the salifiable bases, which in the smallest quantities saturate a given weight of these bases; and the same rule being applied to the attraction of the bases to the acids. The capacity of saturation, however, depends altogether on a different cause,—on the relations of the more remote elements to each other, and not any direct action of acid and base. A larger quantity of barytes is necessary to saturate a given weight of the different acids than of potash, not because barytes has a weaker action on acids than potash has, but because the combining weight of barium is greater than that of potassium, and it combines, therefore, in larger quantity with the radicals of the acids; and conversely, a larger quantity of sulphuric than of carbonic acid is necessary to saturate a given weight of the different bases, not because its affinity to them is less powerful, but be-

* Under these principles, the laws given by Berzelius with regard to the quantity of combined water in acids and in bases, and the proportion which the oxygen in an acid bears to the oxygen of an oxide with which it combines, which some have regarded merely as empirical, and which others have denied, are explained. They follow indeed necessarily from the relations in the combining weights of the elements, when these are considered as in simultaneous combination.

cause

cause the combining weight of sulphur is higher than that of carbon. And were the doctrine of the influence of elective affinities, independent of the operation of external forces on chemical attration, established, barytes would be considered as exerting a more powerful attraction than potash to sulphuric acid, from the attraction of barium to sulphur being stronger than that of potassium to sulphur. From the test, however, of the strength of attraction to be found in the capacity for saturation, the attraction of potassium must be inferred to be superior to that of barium to sulphur; and the results of double decomposition of what are called their saline compounds must be ascribed, in conformity to Berthollet's doctrine, to the influence of the force of cohesion,—this force acting more powerfully on the ternary compound of barium, sulphur and oxygen, than on that of potassium, sulphur and oxygen. These views apply to all the other cases of decomposition in saline combinations.

SUPPLEMENT to the preceding Paper.

SIR H. DAVY has stated some experiments in opposition to the evidence of water being procured from the action of muriatic acid gas on metals*. On these, as far as they refer to the experiments which I executed on this subject†, I may take this opportunity of offering a few observations.

In passing muriatic acid gas through glass tubes ignited, Sir H. Davy found water to be deposited, which he ascribes to the action of the acid on the oxide of lead and the alkali in the glass.

In passing it through glass tubes containing iron ignited, (the experiment I had performed) much more water appeared. "But this he ascribes principally to the combination of hydrogen disengaged from the muriatic acid gas by the iron, with the oxygen of the common air." Any one repeating this experiment will at once be satisfied that this circumstance can have little or no effect in producing the result. The water does not appear until the air has been expelled from the tube by the introduction of the muriatic acid gas; it continues to increase after this, when no air can be supposed present; and the whole quantity of air which the tube could contain, were it even filled with it, is inadequate to afford, by its oxygen, any sensible production of water in such an experiment. The circumstances and result of the experiment which I have described at page 297 of the 8th volume of the Transactions of the Royal Society of Edinburgh, in which the air in the tube had been previously expelled by the introduction of the gas, and that described p. 298, in which

* Philosophical Transactions for 1818, Part I.

† Edinburgh Philosophical Transactions, vol. viii. page 287.

the air had been withdrawn from the retort by exhaustion, altogether preclude this supposition. But its utter inadequacy will, to any one taking the trouble of repeating the experiment, be sufficiently apparent.

It is stated that, in the action of muriatic acid gas on metals, hydrogen equal in bulk to half the volume of the gas is produced; and therefore, it is added, if water had been generated by the action of the acid gas on metals, it must have been the chlorine or the metal, or both, that were decomposed. "But in an experiment of passing chlorine gas over ignited iron wire, not the slightest appearance of moisture was perceptible."

This argument, in common with some others of Sir H. Davy's results, may apply with sufficient force to those experiments in which it is said that water was obtained equal, or nearly so, to the whole quantity of water which, according to the old doctrine, is contained in muriatic acid gas; for it is evident that this water could not have been deposited and hydrogen also evolved. But it does not apply to my experiments, in which a small though very sensible portion of water was obtained; for in such a case hydrogen will also be produced, though not to the precise amount of half the volume. The actual results, therefore, in the particular form of experiment employed, ought to have been ascertained, instead of a general conclusion being reasoned on, more especially when even the general fact is not so clearly established that it can be held as demonstrated. The theoretical result no doubt is, that hydrogen will be evolved equal to half the volume of muriatic acid gas, since the latter is formed from equal volumes of hydrogen and chlorine. But circumstances may occur connected with the experiment, which will modify this. There is one obvious circumstance of this nature,—that of the absorption of a portion of the muriatic acid gas by the muriate formed,—whence, as the entire quantity of acid is not decomposed, the quantity of hydrogen produced must, if the experiment be accurately performed, appear less than half the volume. On this point accordingly there has been considerable diversity of result. Sir H. Davy himself at a former period, in experiments conducted with much care and having no reference to theory, found that the quantity of hydrogen evolved from the action of potassium, and of mercury on muriatic acid gas, is equal only to about one third of the original volume of the gas*. When therefore the conclusion is adopted as the ground of argument that the quantity is one half, without any allusion to any difficulty in the experiment, any source of fallacy attending it, or any opposite result having been obtained, its inaccuracy, or at least its uncertainty, may be fairly presumed. I had already observed in relation to this point, that

* Philosophical Transactions, 1809-10.

if the whole water essential to the acid is decomposed by the action of the metal, half the volume of hydrogen ought to be obtained, muriatic acid gas being composed of equal volumes of oxymuriatic gas and hydrogen gas; while on the other hand, if any additional portion of acid enter into union, besides that forming a neutral compound, the water of this will be liberated, and of course the full proportion of hydrogen will not be obtained. I therefore endeavoured to determine whether this is the case or not; and in repeated experiments, in which iron and zinc acted on muriatic acid gas, the quantity of hydrogen was always less than the half; and on an average, about twelve measures were obtained when thirty measures had been consumed*. It appears, therefore, that in experiments attended with the results I had obtained, that is, when a portion of water is obtained from the action of metals on muriatic acid gas, and a super-muriate is formed, the quantity of hydrogen evolved is not equal to half the volume of the gas consumed; and hence, in reference to these experiments at least, Sir H. Davy's attempt to decompose chlorine was very unnecessary, and the want of success, which it was easy to anticipate, affords no argument whatever.

Muriate of ammonia, it is stated, is not altered by being passed through porcelain or glass tubes heated to redness; but if metals be present, it affords similar results to muriatic acid gas, and the water obtained is ascribed to the action of the hydrogen liberated from the acid and from the ammonia on the oxide of lead in the glass. In the experiments of which I have given an account (*Edinburgh Transactions*, vol. viii. p. 301) I found that exposure to a heat not so high as that of ignition is not necessary to obtain water from the action of metals or muriate of ammonia; one much more moderate, and at which no action on the glass can be supposed to be exerted, is sufficient; and accordingly, not the slightest indication of the glass being acted on can be perceived. In obtaining, for example, water from a mixture of tin filings and muriate of ammonia heated in a retort by the gentle heat of a small lamp, the retort remains perfectly unaltered, in colour, transparency, and lustre.

These objections, then, I regard as of no force; at the same time I do not consider the discussion as of much importance. The view which I have now proposed of the nature of muriatic acid does not rest on any exclusive proof of water being obtained from it, but on other grounds; and it is quite sufficient that it yields water in the same cases of chemical action, in which other powerful acids, as the sulphuric, nitric and oxalic, afford it, while the sulphurous and carbonic afford none. The same theory applied to the constitution of the former will fall with every proba-

* *Transactions of the Royal Society of Edinburgh*, vol. viii. p. 307.
Vol. 54. No. 257. Sept. 1819.

bility to be applied to that of muriatic acid; and whatever superiority may belong to it, this will be applied to both. The question, therefore, deserves attention only on the principle, that in chemical investigations it is always of importance to adhere rigidly to the observation and strict expression of a fact, whether it is conformable to a prevalent doctrine or not, or whether it admits of obvious explanation or not, on any established law. In numerous experiments on muriatic acid gas I have always obtained water in small but very sensible quantity, where its production, I am satisfied, cannot be accounted for from any of the extraneous sources to which it has been attempted to refer it. And I certainly shall not refrain from maintaining what I regard as the strict expression of an experimental result; at the same time, in the experiments at present referred to, the formation of a supermuriate affords a principle, which, as I have already stated, sufficiently accounts for the fact.

XXXIII. *Remarks on Madeira, Climate of the Tropics, Trade-Winds, Rio Janeiro, the Polar Ice, &c. Extracted from a Journal kept by JOHN HAMMET, M.D. Surgeon R.N., in a Voyage from England to Rio Janeiro. Communicated by Dr. PEARSON.*

[Concluded from p. 117.]

ON the 23d of January we left Rio Janeiro, *sine privilegio regali minime gentium*, in order to proceed to Ribeira in the district of Ubatuba, to take in our lading of timber, the last granted perhaps for many years to come, and anchored at 7 P.M. of Monday the 25th in four fathoms, a little round a biangular projection to the left in Sharks' Bay. Between Rio de Janeiro and the village of Ubatuba the land is in general bold and high, abounding with impendent precipices above*, and rugged declivities between these and naked rocks half buried in the sea below; but between Ubatuba and the village of St. Sebastian, on the main land and opposite the island of the same name, it is a continued series of bays, of various sizes and forms, running in different directions.—The πολυφλοίσβοις θάλασσα, so accordant to the venerable Chryses' or our own reverend vicar's feelings, is outdone by the loud surges of the multitudinous billows of the Southern Ocean rolling and dashing in succession with the greatest impetuosity and violence against the long and level sandy beaches in this part of the Brasils.

The short sail from Rio de Janeiro to Ribeira was certainly

* “ ———— *Rupes et acuta letho
Saxa.*”

pleasant in the extreme. The fragrance of the vegetable creation of South America is well known, and its influence I must for my own part say I experienced, owing to the moderate breezes that prevailed, the pleasantness of the weather, and our proximity to the land. In consequence of the number of bays along this coast, we had occasion on our passage to anchor for a while between the islands of Vittoria and Briseas in this direction, in order to ascertain the situation of the bay to which we were bound. The whole of these circumstances were calculated to excite cheerfulness, and conspired to remind me of that beautiful simile of Milton's, in the fourth book of his *Paradisus Perditus*, so well known to the Jesuits in Du Halde's time :

“ As when to them who sail
Beyond the Cape of Hope, and now are past
Mozambic, off at sea north-east winds blow
Sabeian odours from the spicy shore
Of Araby the blest ; with such delay,
Well-pleased they slack their course, and many a league
Cheer'd with the grateful smell old Ocean smiles.”

There certainly is in this part of the Brasils much left yet for the patient research of the botanist and naturalist :—Here are still on every side, as well as back in the mountains, woods *

“ Where the rude axe with heaved stroke
Was never heard the nymphs to daunt,
Or fright them from their hallow'd haunt ;”

or, in reality, where ounces, sloths, black tigers, guanicos, and non-descript animals range at large, without molestation and without danger ; and whence the ravenous *ouraboos* instinctively wing their way in flights, into distant plains, to devour dead animals and every species of carrion that otherwise would tend to render the air pestiferous. Of these, the low swampy tracts are more particularly the abode of the *syrakoora*, the natural enemy of noxious insects ; the *frange de aguu* and *tesoura*, unobservant of each other, keep in those parts near the water, and occasionally frequent marshy places ; the *synumboo* is more accustomed to excursion than to aerial flights ; and the wild *makook* and shy and rare *jakoo* keep exclusively to woods situate on hills and mountains. Besides various others of a novel and singular nature, there are many hitherto unknown, which from their solitary abodes, circumspective habits, and instinctive dread of ruthless man, are likely to continue so for generations yet to come. In these also are insects of all hues and kinds ; some as beautiful as they are harmless, and some as insidious as they are venomous.

* ———— *Ut mihi devio
Ripas et vacuum nemus
Mirari libet.”*

Here also the beetles act in stunning concert at night, while the creeping things hurtful and unhurtful venture out with impunity and in safety.

We departed from Sharks' Bay on Tuesday the 23d of February at 5 A.M., and at 7 P.M. the same day anchored opposite Villa Velha Princessa, in the island of St. Sebastian, in seventeen fathoms, in the channel between it and the main land, or *terra firma*, as the Portuguese-Brasilians very properly term it. In this place, which is remarkable for thunder and lightning, we witnessed a violent thunder-storm on the second night after our arrival. Here we completed our lading, and took our departure from it for Rio de Janeiro on Friday the 12th of March at 10 A.M., at which we again arrived on Friday the 19th of the same month at noon, and finally left it for England on Saturday the 24th of April at 6 A.M. We crossed the equator in $20^{\circ} 30'$ west longitude, in the first watch (at night) of Saturday the 22d of May; on which day at noon the temperature was, with moderate breezes from the S.W., at 86° : and on Sunday the 30th, in $3^{\circ} 26'$ north latitude and $18^{\circ} 54'$ west longitude, it was, with light and variable airs at noon, up to 94, the highest that I remarked it between the tropics on our way out and home this voyage. We crossed the tropic of Cancer in $36^{\circ} 15'$ west longitude, between 9 and 10 at night, the 16th of June, on which day, at noon, the temperature was, with lowering dense clouds and fresh easterly breezes, only at 76, notwithstanding the vertical position of the sun at the same time. And between the tropic at this meridian and the unusual extent of the trade-winds to the parallel of 44° north and the meridian of 38 west, this month (as already stated) the temperature was only between 77 and 72:—so unstable are the elements, that Bacon might well cry out in his sounding and emphatic gerunds—“*non fingendum, aut excogitandum, sed inveniendum, quod natura faciat aut ferat.*” At 72° we all at first thought it extremely cool; a proof that animal sensation, depending, as it entirely does, on previous susceptibility, that is continually varied by every passing influence, has little relation in the admeasurement of the absolute matter of heat. Between this and the latitude of $42^{\circ} 23'$ north and longitude $23^{\circ} 56'$ west, to which we arrived on the 2d of July at noon, the temperature was, with prevailing breezes from the southward, between 73 and 69; and between this and the latitude of $45^{\circ} 51'$ north and $16^{\circ} 40'$ west, to which we arrived on the 6th of July at noon, the temperature was, with prevailing breezes from the northward, between 60 and 56; and from this to the soundings in Channel, to which we have just arrived, the 10th of July, after a tedious passage of eleven weeks, it has been, with prevailing breezes from the westward, and with an *apparently*

rently frosty sky and foggy weather, rather higher. The degree of temperature, as mentioned here, is to be referred to the time between the hours of eight and eight in the day. These are only a few of the innumerable instances of the extraordinary mutability of temperature, to which not only sea-faring men but sea-faring patients, in traversing distant seas, are in general exposed.

On the 17th, 18th, and particularly the 19th of June, while near the tropic, and a little to the westward and eastward of $36^{\circ} 15'$ west longitude, we fell in with considerable detached pieces of sea-weed, resembling a small white kind of berry, that must have made their way with the current drifting in this direction out of the Gulf of Florida.

While near the equator on our return, and as far as 18° west longitude and $7^{\circ} 45'$ north latitude, there were incessant torrents of rain with prevailing calms and occasional squalls; among which was remarked the *grain blanc*, formerly noticed by the ingenious and unfortunate French circumnavigator. To steer clear of these tremendous and highly sickening torrents, it is necessary to cross the equator on the western side of, at least, its 25th degree, in the same direction; because, as the sea-breeze, occasioned by the dry and heated continent of Africa, springs up at an immense distance from the land, and the trade-winds commence in the same or contiguous parts, the atmosphere, from its being thus drained or attenuated, suffers an incessant or instantaneous descent of the vapours or clouds continually going on or occasionally impelled thither from the remoter parts.

In the horizons of these trackless regions are still to be seen *slave ships* stealing along to the Portuguese, French and Spanish settlements, notwithstanding all that has been said and done concerning this iniquitous and infernal traffic. On Sunday the 6th of June, at about nine at night, in $7^{\circ} 25'$ north latitude and $23^{\circ} 16'$ west longitude, we fell in with, and detained until about the same hour the following morning, the *Roger* (French slave-ship) from Havre de Grace, having 230 negroes on board, consisting of men, women and children, and bound from the river Gabon to Havana, or the island of St. Thomas. She was out 48 days, lost her reckoning, was considerably short of water; all her crew but three affected with a malignant ophthalmia, and the hapless creatures devoted to slavery like to have their fate averted in the successive deaths, that daily or frequently occurred, in consequence of confinement, and, as I was informed, privations in the mere common necessities of subsistence, which, for the honour of human nature, I hope was not the case. At any rate, three of their dead bodies were committed to the deep early in the above morning of our separation.—Surely those barbarous

and speculating agents, who dare risk imperfect and uncertain equipments of this kind, are guilty of the manifold crime of wilful murder.—Finding that my services were not solicited on this calamitous occasion, and learning that the person in the capacity of surgeon on board her was almost entirely blinded with the disorder, I could not refrain from officiously proffering both my advice and assistance. This unfortunate man, and the master of the vessel, I found in despair on account of their malady, which, with that of all but three of the crew, who were, as I have just stated, similarly affected, seemed to me to be of a highly inveterate and formidable nature, owing chiefly to their having no saturnine or astringent *collyria*, or, in fact, efficient applications of any kind, and otherwise to imperfect and consequently to ineffective treatment, which consisted simply of ablutions with cold water applied with dirty rags, and the application of blisters to the nape of the neck alone.—Respecting the history of this affection, I could learn nothing further, than that it broke out in one or two of the men just before the vessel left the river; and in consequence of confinement from the heavy and protracted falls of rain, and the consequent want of ventilation and cleanliness, and, in all probability, a similarity of predisposition among the crew, it continued rapidly to spread and to increase in inveteracy; which, under those circumstances, induces me to suppose it infectious or contagious, or infectious and contagious, and as such to be dreaded and avoided. Here it is necessary to state, that under existing circumstances the Coromandel could not well detain this vessel, at such an immense distance from any port as that implied.

During the intervals of our having remained at Ribeira and the island of St. Sebastian, the sick-list contained between 15 and 18 on an average. The disorders were chiefly inflammatory fevers, and *infectious* dysenteries. Much the greater part of the timber was lodged in an angular recess round the projection already mentioned, as situate to the left of Sharks' Bay, a place entirely shut in from the sea-breeze; so that in addition to this circumstance, if there be considered the nature of the duty itself under a vertical sun, requiring an immersion of one half of the body, and likewise, from its plenty and cheapness, the free use of the *agoar dentè de cana*, or *de cachaça**, and the consequent improvident exposure of the body, by night as well as by day,

* This is almost as indigestible as the *koumis* of the Baschkirs in Asiatic Russia, specified in your (Dr. Pearson's) Lectures, Sir; for as the one, from its indigestible and stimulating nature, retains a considerable share of its inebriating properties after it takes a "*north-east*" passage through the system, so does the other remain in some instances little changed after it takes a "*north-west*" course through it.

under a highly and peculiarly influencing atmosphere, notwithstanding the strictest attention to the judicious regulations in these respects, the prevalence of these disorders will not appear at all wonderful. There were, I am happy to inform you, no deaths; on the contrary, from prompt and active treatment, and the favourable circumstance of a clean ship with large port-holes admitting every possible degree of ventilation her position would admit, the fever patients became convalescent in about eight days, and the dysenteric patients in about a fortnight or three weeks at most.

In reference to my mode of treating dysentery, you will find it sufficiently exemplified in the subsequent part of these observations and reflections; and in relation to my treatment of inflammatory fevers, I will freely confess to *you*, that it was on the Sangradian principle, inasmuch as it implies promptitude in sanguineous evacuations, and unrestraint in the use of tepid drinks; with this difference, however, that the drinks administered by me were of a starchy nature, or of composing and refreshing virtues, and were intended, in the first instance, to promote the speedy effects of drastic remedies exciting the peristaltic action of the glandular and seriferous extreme vessels in the internal coats of the intestines.

On this occasion, I cannot, by the way, refrain from remarking that Le Sage's account of Sangrado is not so much a lampoon, a caricature, or a satire, as it is a gross and scandalous libel against the faculty in general. This, I think, is fully evinced in his invectives throughout the whole of his justly celebrated *Gil Blas*, in the evidently falsified statement of deaths, occasioned not by him alone, but by physicians in general, that could be written with the view only of dishonouring the profession, and in the opprobrious epithet bestowed on the ostensible personage in that work, his object of attack on every possible occasion. As the Spaniards are remarkable for native intellect, it is not unreasonable to suppose that Sangrado, perhaps, was some great and original genius, whose conspicuity of character alone subjected him to the libellous denunciation so artfully displayed by this writer, notwithstanding his prefatory asseveration to the contrary, and to which he could have been incited only by the prevailing ignorance or prejudice of the times; a prejudice that pervaded even the monastic orders of both sexes, among whom an unwillingness to admit any of the laity, however qualified, to inspect into their complaints, or to administer to their relief, was invariably manifested; since with Le Sage's libel, in many instances notoriously ludicrous, not a few of the monks of that time vainly cooperated with their stillborn censure; whose motives are conceivable, but may not be expressed.

Cervantes, Fielding, Marivaux, and Smollet, the superiors in my humble opinion of Le Sage, have shown better judgements on this head. Smollet, who on *more* accounts than one was the best qualified in this respect, saw the proper openings for satire, admirably entered into and traced them.

That first-rate broker of episodes, however highly gifted he was to retail to the world the wiles and hypocrisy of common life, open alike to all mankind, was certainly unqualified to dive into the mysteries of the medical profession, or to decry its professors. Le Sage, not having studied and practised physic himself, could be in nowise authorized to judge of the unfitness of evacuations, or to infer death as their necessary consequence. This principle is finely enforced by numerous writers of the highest rank, and by none more than by Homer, Horace, Pope, and Goldsmith, who, in particular, with his usual *naïveté* inveighs against Peter the Great, and Locke, notwithstanding the powers of his mind, and his partial attention to physic, for meddling with the principles of a profession that can be radically learned only by a knowledge of matter and nature in general, and by an intimate acquaintance with the living and dead subject in every stage and in every state. It is however well known that Le Sage lived to recant what he had said in dispraise and derision of a profession coeval with fallen man, and whose genuine professors have been ever honoured by the first writers and characters of every age and country, on account of their indispensable utility, and who, in the opinion of Swift *, should not only take precedence of doctors of the canon or civil law, but should be ranked in the first classes of the community, instead of the lower order of nobility, as is on extraordinary occasions the case.

People living in a state of mere nature will experience the wants and dependencies of man, and privations will necessarily prevail in all colonies at first. In those parts of the Brasils where I have been, they seemed to want the skill, or require the means, necessary to render the mandible produce of the soil most conducive to digestion, or, strictly speaking, to chymification, chyli-fication, sanguification, and assimilation. Their staple article of food is *farinha*, a coarse meal made from the root of the *mandioca* or *casada* plant †. It is made use of simply in its dry state with water, after or on being mixed up in a decoction of fish,

which

* One of the *three* original writers in modern times, according to Voltaire. Again, the curious reader can find what Blackstone has said in praise of medical men. Thus we perceive a first-rate doctor of the canon law avowedly, and a first-rate doctor of the civil law indirectly, yielding the palm of greatest honour to the genuine medical professors.

† Each family that I happened to notice has a handmill, with which the root, on being well scraped and washed, is ground; after which it is pressed and

and

which together commonly constitute their principal meal. The middle sort, however, make use also of *feijao* or black beans, and occasionally fowl, &c. with *agoar dentè* as a corrective. Their breakfast generally consists of *farinha* and coffee. At any rate, it must be partly owing to the nature of the food made use of, or to the manner of dressing it, as well as to the nature of the climate, that scarcely an individual is to be met with, who is not affected with some of the prominent symptoms ostensive of dyspepsia, and that by far the greater number of the inhabitants are variously and grievously affected. Persons who live in solitude will find the ravaging effects of disease, particularly within or near the torrid zone—

“ ————— *sub curru nimum propinqui*
Solis, in terra domibus negata.”

For this reason, and in order to exclude misery from such, his favourite state, the inimitable St. Pierre, in one of the most exquisite paintings that the love of nature and of virtue ever inspired, has not forgotten to particularize the use of medicine.

Here the inhabitants have, or pretend to have, a high opinion of English physicians, which, with the remoteness of *Rio Janeiro* and *San Paulo*, the nearest places where medical men of any description can be found, caused such as were diseased to flock from all the contiguous parts to the ship, for cure or relief; so that I may be allowed to say concerning the diseases of these parts,—“*haud inexpertus loquor.*”

I have often admired that excellent passage in Shakspeare's works, so remarkable for its physical justness, viz.

“ ————— Life,
————— a breath thou art,
Servile to all the *skye* influences,
That do this habitation where thou keep'st
Hourly afflict.”

“ ————— Thou art not certain;
For thy complexion shifts to strange effects,
After the moon.”

From this last paragraph, in particular, I am led to remark that diseases have of late been very generally referred to a superficial or first-sight doctrine of induction alone; and a local modification, without any due regard to what produces it, has been as generally considered the sole exciting cause. Thus Dr. Parr of Exeter, in his remarks on the influence of the heavenly bodies, states, that diseases may be referred to causes less remote.

and washed, in order to extract a poisonous juice from it, and then put into a kind of kiln and dried.—In the liquor, which runs through in the press, a sediment forms, which, on being washed and levigated, constitutes the well known *tapioca*.

For my own part, I really wonder how a man of his vast medical lore, and extensive erudition, could possibly hazard such an illogical assertion, when, in nine instances out of ten, those causes are manifestly consequences depending on, or connected with, those remote ones. Dr. James Johnson, again, an eminent surgeon in the navy, and a gentleman entitled to great praise and respect on account of his industry, abilities, and services, attacks Dr. Mosely *omnibus nervis suis* for his Canidian doctrine of lunar influence. Had Dr. Mosely, however, been only attacked here, he certainly would have proved invulnerable.—Shades of Boerhaave, of Mead, of Arbuthnot, and of the Gregorys! your doctrines are decried, and about to be exploded, not by such men as these alone, but by others, who cannot trace the synthetic links of the great chain of atmospheric influence, through causes and effects, and who seem to grope and tread in thorny confusion, and finally, with vexatious disappointment, to look up and exclaim against the goodly tree. What, in the name of science, are the seasons of the year, and the morning and the evening, and the noon and night, of the diurnal period, but modifications of the sun's influence on the atmosphere and earth, according to the particular positions of the latter in the ecliptic, and (notwithstanding any abrupt assertions to the contrary) more or less varied by the influence of the moon, according to her relative positions in conjunction and opposition, and in *perigæo* and *apogæo*, in her intricate motion through her zigzag* orbit? For the truth of the varied difference of animal sensation, from the particular state of the weather induced by the influence of the heavenly bodies, I could, in point of evidence, appeal even to the feelings of the wandering Tartar or apathetic Indian; but, authorized as I am, in so clear a case, to judge and decide, I will simply state that there is no patient, valetudinarian, or person of sensibility, who has not, in a high degree, experienced gladdening and depressing sensations from the vicissitudes of the seasons and the changes of the weather; and that these are only modified states of the atmosphere, by which, from the conjunct influence of the sun, moon, and earth, we live and breathe and get our being.—Concerning planetary influence, as it is, in a medical sense, certainly too remote, I have nothing to offer; and shall leave it to the votaries of horoscopes, and to the admirers of the celebrated Dr. Dee, and Farquhar's inimitable astrologer, who placed, with such dexterous aptitude, "Forceps, Furnes, Dixmude, Namur, Brussels, and Charleroy," among the constellations in the zodiac, there to remain as long as any placed by the

* What honours are due to the memory of Mayer and Euler, as well as Newton and La Place, on this head!

learned Dr. Halley, or by Ovid, or any of the poets of antiquity, elsewhere beyond the Goat and Crab.

Ludicrous digressions apart: It is, however, certain that every climate, and every change in it, has its specific and relative influence on man; and thus it is that every place has its endemic or prevailing diseases, favoured, however, by accidental and local circumstances. In England, for instance, the density of the atmosphere, the prevailing humidity, and occasional siccidity of it, and the mutability of temperature, are the exciting causes to rheumatisms, asthmas, catarrhs, and pulmonary disorders, so prevalent in it, and that afterwards, on their occurrence, prove inimical to their cure. On the contrary, in climates intensely hot, as Rio de Janeiro, and those parts of the Brasils already mentioned, the atmosphere is remarkable for its tenuity, its mildness, and its comparative inequality of temperature; and these disorders, it is true, rarely take place, unless in extraordinary instances of exposure by night, or unless among the Negroes, who go about almost naked in the sun, and in this state at night stretch their fatigued bodies on the damp ground.—Again, that gloom or heaviness of the mind, which so frequently seeks refuge in self-destruction in England, is in those climates utterly unknown. That starting from sleep, peculiar to melancholic serious or irritable minds, and common in cold, moist, and heavy atmospheres, and the occasional intrusion, at the same moment, of horrid anxiety for one's own or family's condition, or of the varied "dreadful thought" of "eternity," are also, in a physical sense, unknown. It is true that in those climates the mind is notoriously influenced by the languor of the body; still equanimity prevails amid the depression of the spirits, and the mind has nothing of that northern dejected and repulsive, or compassionated cast, alluded to, about it. Hence we perceive, that although in such climates as the Brasils the mind is liable to, and does actually suffer, amid the prevalence of a variety of diseases, and particularly of dyspepsia, from the manifest nervous connexion between the stomach and brain, through the medium chiefly of the *par vagum* and *nervus sympatheticus maximus*; we however perceive, that a moist, dense and cloudy atmosphere alone has a peculiar influence on the mind *per se*, through the sympathetic and immediate agency of the nervous system, and more particularly the primary source of it, the brain. But then, on the other hand, as I have just intimated, the Brazilian climates are notorious for the prevalence of the whole train of symptoms constituting the disease of dyspepsia, and all those diseases connected with the glandular system;—for torpor, congestions or infarctions, as well as inflammation of the liver;—for elephantiasis;

elephantiasis; erysipelatous legs; every species of caligo; hypogastric morbid tumours, long before and after the final menstrual cessation; swelled testes, chiefly spermatocœles in the first instance; and particularly for bringing on, with occasional circumstances unconnected with contagion, the disease of dysentery; on which accounts, and its not being, as is commonly erroneously imagined, propagated from person to person, I have considered it infectious, not contagious.—Here, admitting for a moment that the disease of dysentery is contagious and not infectious, as is commonly supposed to be the case in camps and ships, and that it broke out among twelve persons there, and that the twelfth received it from the eleventh, and the eleventh from the tenth, and so on, until the second received it from the first: how did it arise in him? Is it from το Θείον of Hippocrates? In short, our definitions of it are erroneous; and this *quid pro quo* is frequently nothing more than a similarity of predisposition among bodies congregated together, which subjects each to the same atmospheric influence. But to return.—Again, in high temperatures not mutable, infectious influences prevail, and diseases are produced in consequence of the stagnant effluvia, which necessarily exist in such a state, and in consequence of the concurring agency of electricity in the same. Thus, in places remarkable for the plague*, a high temperature is generally known to prevail, without any evident change in it, while, at the same time, the silent and morbid agency of electricity can be obviously traced; so that, in a physical as well as in a moral sense,

“Heaven’s just balance equal will appear.”

From the whole of what has been just stated, it may be briefly inferred, that the former is inimical chiefly to the thoracic, and the latter to the abdominal viscera; and that persons affected with any of the thoracic complaints prevalent in England or similar climates, particularly tubercles and vomicae, are, from the dangerous influence of the torrid climates, also liable to become affected with fevers, diarrhoeas, and more especially with dysentery, which above all will not fail to aggravate the former, and in many instances fatally.

* Epidemics of this nature can only get birth in northern or southern climes, under the circumstance of an incidentally high temperature, favoured by local and other circumstances; as in the particular instance of the ravaging plague in London; of which we have a *fictional account* by the ingenious author of Robinson Crusoe, who, it appears, could, like Cowley and Hawkesworth, deal out fiction better by wholesale, than plain matter of fact by retail.

XXXIV. *Observations on certain free Remarks by Mr. FAREY published in the last Number of the Philosophical Magazine. By G. B. GREENOUGH, Esq.*

To Mr. Tillock.

SIR, — **I**N the last Number of your Magazine, Mr. Farey complains with justice that a passage, extracted from his Agricultural Report of Derbyshire, is, in a work which I have lately published, ascribed, not to its real author, but to Mr. Hutchinson. I am sorry for the error, and still more that, not being previously informed of it, I have had no opportunity of cancelling till now the leaf that contained it. To say that the error was unintentional, is scarcely necessary. This must be obvious to every one who does not consider me destitute of common sense as well as common honesty.

That I ascribe to Mr. Hutchinson the words of Mr. Farey, is however not my only offence. I am accused of not ascribing to Mr. Farey the discovery* of Mr. Hutchinson. I am accused also of not quoting books which I had not read. Both these charges, like the former, are perfectly just.

Your correspondent considers me, in common with many other persons, actuated by feelings of hostility towards Mr. Smith. Now my feelings towards that gentleman are directly the reverse. I respect him for the important services he has rendered to geology, and I esteem him for the example of dignity, meekness, modesty, and candour, which he continually, though ineffectually, exhibits to his self-appointed champion.

In the work of Dr. Lister, entitled *Historia Animalium Angliæ*, is a passage which I have alluded but not referred to, "for reasons," it is said, "best known to myself." Your Correspondent has discovered this passage, notwithstanding all the pains I took to conceal it, and, being in Latin, has got it translated by a gentleman whom, with a proper sense of gratitude for the service conferred upon this occasion, he characterizes as a learned naturalist and kind friend. That he is a kind friend I readily believe; for, not content with misconstruing the last clause in the sentence, he does not construe at all the words "*at perpetuo*," by far the most important of any, as affecting the question at issue. The question is, whether Lister did or did not perceive a connexion between the small belemnite and the bed which con-

* In your Magazine, vol. xlii. p. 107, Mr. Farey, referring to the case of faults deranging the strata beneath, but without a corresponding step or cliff appearing on the surface, informs us that this fact was first pointed out by Mr. John Hutchinson; in your last Number he says "Mr. Greenough well knows that in 1806 I made the important discovery."

tains it. The original passage is, "Hunc lapidem (viz. the belemnite) plurimis in locis apud nos quam copiosissimè inveni; *at perpetuo* in terrâ rubrâ ferreâ *sive ea mollior gleba sive saxeâ sit.*" Now follows the translation: "This stone is found very abundantly in many places among us in a red ferruginous earth, either in softer or more stony masses."

In one of my Essays I have mentioned the name of Mr. Martin first, and that of Mr. Smith afterwards, to the great displeasure of my commentator, who thinks I should have mentioned the name of Mr. Smith first, and that of Mr. Martin afterwards; for what reason I am utterly at a loss to imagine, unless, because in reading the passage my commentator has overlooked the words "at an early period," as his friend has overlooked the "*at perpetuo.*" I really feel obliged to one, who, in questioning my fairness, affords so characteristic a specimen of his own.

In the same volume I have adduced my reasons for doubting the intimate and invariable connexion which has been said to exist between contemporaneous strata and their fossils. Mr. Farey, an advocate for that connexion, which he maintains was first laid down by Mr. Smith, does not undertake to prove its correctness; he contents himself with assuming it. But as the assumption of a contested fact is apt to imply, not the absence of doubt, but the absence of evidence, I am somewhat curious to be informed how far Mr. Farey's theory on this subject is borne out by his experience. He has examined Derbyshire with very laudable industry; will he take the trouble to mention, what the fossils are, by which he is enabled to distinguish the different limestones in that county, or the different sandstones, or the different shales? There will be time to discuss the originality of the doctrine when its truth is established. If its truth cannot be established, I beg very respectfully to ask Mr. Farey, whether he can hope to exalt the character of his teacher by proving him the first discoverer of that which does not exist?

I am, sir, your obedient servant,

G. B. GREENOUGH.

XXXV. *A new Theory of Galvanism, supported by some Experiments and Observations made by means of the Calorimotor, a new Galvanic Instrument; also, a new Mode of decomposing Potash extemporaneously. Read before the Academy of Natural Sciences, Philadelphia. By ROBERT HARE, M.D. Professor of Chemistry in the Medical Department of the University of Pennsylvania, and Member of several Learned Societies.*

I HAVE for some time been of opinion that the principle extricated by the Voltaic pile is a compound of caloric and electricity,

tricity, both being original and collateral products of Galvanic action.

The grounds of this conviction, and some recent experiments confirming it, are stated in the following paper.

It is well known that heat is liberated by Voltaic apparatus, in a manner and degree which has not been imitated by means of mechanical electricity; and that the latter, while it strikes at a greater distance, and pervades conductors with much greater speed, can with difficulty be made to effect the slightest decompositions. Wollaston, it is true, decomposed water by means of it; but the experiment was performed of necessity on a scale too minute to permit of his ascertaining, whether there were any divellent polar attractions exercised towards the atoms, as in the case of the pile. The result was probably caused by mechanical concussion, or that process by which the particles of matter are dispersed when a battery is discharged through them. The opinion of Dr. Thomson, that the fluid of the pile is in quantity greater, in intensity less, than that evolved by the machine, is very inconsistent with the experiments of the chemist above mentioned, who, before he could effect the separation of the elements of water by mechanical electricity, was obliged to confine its emission to a point imperceptible to the naked eye. If already so highly intense, wherefore the necessity of a further concentration? Besides, were the distinction made by Dr. Thomson correct, the more concentrated fluid generated by a Galvanic apparatus of a great many small pairs, ought most to resemble that of the ordinary electricity; but the opposite is the case. The ignition produced by a few large Galvanic plates, where the intensity is of course low, is a result most analogous to the chemical effects of a common electrical battery. According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air, with immeasurable celerity, and, distributing itself in the interior of bodies, communicates a reciprocally repellent power to atoms, but not to masses. Electricity does not radiate in or through any matter; and while it pervades some bodies, as metals, with almost infinite velocity; by others it is so far from being conducted, that it can only pass through them by a fracture or perforation. Distributing itself over surfaces only, it causes repulsion between masses, but not between the particles of the same mass. The disposition of the last mentioned principle to get off by neighbouring conductors, and of the other to combine with the adjoining matter or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification

cation of their properties, consequent to some such union, they could not, in piles of thousands of pairs, be carried forward through the open air and moisture; the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric.

Pure electricity does not expand the slips of gold leaf, between which it causes repulsion, nor does caloric cause any repulsion in the ignited masses which it expands. But as the compound fluid extricated by Galvanic action, which I shall call electro-caloric, distributes itself through the interior of bodies, and is evidently productive of corpuscular repulsion, it is in this respect more allied to caloric than to electricity.

It is true, that when common electricity causes the deflagration of metals, as by the discharge of a Leyden jar, it must be supposed to insinuate itself within them, and cause a re-action between their particles. But in this case, agreeably to my hypothesis, the electric fluid combines with the latent caloric previously existing there, and, adding to its repulsive agency, causes it to overpower cohesion.

Sir Humphry Davy was so much at a loss to account for the continued ignition of wire at the poles of a Voltaic apparatus, that he considers it an objection to the materiality of heat; since the wire could not be imagined to contain sufficient caloric to keep up the emission of this principle for an unlimited time. But if we conceive an accumulation of heat to accompany that of electricity throughout the series, and to be propagated from one end to the other, the explanation of the phænomenon in question is attended by no difficulty.

The effect of the Galvanic fluid on charcoal is very consistent with my views, since, next to metals, it is one of the best conductors of electricity, and the worst of heat, and would therefore arrest the last, and allow the other to pass on. Though peculiarly liable to intense ignition when exposed between the poles of the Voltaic apparatus, it seems to me it does not display this characteristic with common electricity. According to Sir Humphry Davy, when in connexion with the positive pole, and communicating by a platina wire with the negative pole, the latter is less heated than when, with respect to the poles, the situation of the wire and charcoal is reversed. The rationale is obvious: charcoal being a bad conductor, and a good radiator, prevents the greater part of the heat from reaching the platina, when placed between it and the source whence the heat flows.

I had observed that as the number of pairs in Volta's pile had been extended, and their size and the energy of interposed agents lessened, the ratio of the electrical effects to those of heat had increased; till in De Luc's column they had become completely
predominant;

predominant; and, on the other hand, when the pairs were made larger and fewer, (as in Children's apparatus,) the calorific influence had gained the ascendancy. I was led to go further in this way, and to examine whether one pair of plates of enormous size, or what might be equivalent thereto, would not exhibit heat more purely, and demonstrate it equally with the electric fluid, a primary product of Galvanic combinations. The elementary battery of Wollaston, though productive of an evanescent ignition, was too minute to allow him to make the observations which I had in view.

Twenty copper and twenty zinc plates, about nineteen inches square, were supported vertically in a frame, the different metals alternating at one half-inch distance from each other. All the plates of the same kind of metal were soldered to a common slip, so that each set of homogeneous plates formed one continuous metallic superficies. When the copper and zinc surfaces thus formed are united by an intervening wire, and the whole immersed in an acid or aceto-saline solution, in a vessel devoid of partitions, the wire becomes intensely ignited; and when hydrogen is liberated it usually takes fire, producing a very beautiful undulating or coruscating flame.

I am confident, that if Volta and the other investigators of Galvanism, instead of multiplying the pairs of Galvanic plates, had sought to increase the effect by enlarging one pair as I have done, (for I consider the copper and zinc surfaces as reduced to two by the connexion,) the apparatus would have been considered as presenting a new mode of evolving heat as a primary effect independently of electrical influence. There is no other indication of electricity when wires from the two surfaces touch the tongue, than a slight taste, such as is excited by small pieces of zinc and silver laid on it and under it, and brought into contact with each other.

It was with a view of examining the effects of the proximity and alternation in the heterogeneous plates, that I had them cut into separate squares. By having them thus divided, I have been enabled to ascertain, that when all of one kind of metal are ranged on one side of the frame, and all of the other kind on the other side of it, the effect is no greater than might be expected from one pair of plates.

Volta, considering the changes consequent to his contrivance as the effect of a movement in the electric fluid, called the process electro-motion, and the plates producing it electro-motors. But the phenomena show that the plates, as I have arranged them, are *calori-motors*, or heat-movers, and the effect *calori-motion*. That this is a new view of the subject, may be inferred from the following passage in Davy's Elements. That great

chemist observes, “ When very small conducting surfaces are used for conveying very large quantities of electricity, they become ignited ; and of the differet conductors that have been compared, charcoal is most easily heated by electrical discharges*, next iron, platina, gold, then copper, and lastly zinc. The phænomena of electrical ignition, whether taking place in gaseous, fluid, or solid bodies, always seem to be the result of a violent exertion of the electrical attractive and repellent powers, which may be connected with motions of the particles of the substances affected. That no subtile fluid, such as the matter of heat has been imagined to be, can be discharged from these substances, in consequence of the effect of the electricity, seems probable, from the circumstance, that a wire of platina may be preserved in a state of intense ignition *in vacuo*, by means of the Voltaic apparatus, for an unlimited time ; and such a wire cannot be supposed to contain an inexhaustible quantity of subtile matter.”

But I demand, where are the repellent and attractive powers to which the ignition produced by the Calorimotor can be attributed? Besides, I would beg leave respectfully to inquire of this illustrious author, whence the necessity of considering the heat evolved under the circumstances alluded to as the effect of the electrical fluid ; or why we may not as well suppose the latter to be excited by the heat? It is evident, as he observes, that a wire cannot be supposed to contain an inexhaustible supply of matter, however subtile ; but wherefore may not one kind of subtile matter be supplied to it from the apparatus as well as another? especially, when to suppose such a supply is quite as inconsistent with the characteristics of pure electricity, as with those of pure caloric?

It is evident from Mr. Children’s paper in the *Annals of Philosophy*, on the subject of his large apparatus, that the ignition produced by it was ascribed to electrical excitement.

For the purpose of ascertaining the necessity of the alternation and proximity of the copper and zinc plates, it has been mentioned that distinct square sheets were employed. The experiments have since been repeated and found to succeed by Dr. Patterson and Mr. Lukens, by means of two continuous sheets, one of zinc, the other of copper, wound into two concentric coils or spirals. This, though the circumstance was not known to them, was the form I had myself proposed to adopt, and had suggested as a convenient for a Galvanic apparatus to several friends at the beginning of the winter† ; though the consideration above

* The conclusions are drawn from experiments made by the electricity of the Voltaic apparatus.

† Especially to Dr. T. P. Jones, and Mr. Rubens Peale, who remember the suggestion.

stated induced me to prefer for a first experiment a more manageable arrangement.

Since writing the above, I find that when, in the apparatus of twenty copper and twenty zinc plates, ten copper plates on one side are connected with ten zinc on the other, and a communication made between the remaining twenty by a piece of iron wire about the eighth of an inch in diameter, the wire enters into a vivid state of combustion on the immersion of the plates. Platina wire equal to No. 18 (the largest I had at hand) is rapidly fused if substituted for the iron.

This arrangement is equivalent to a battery of two large Galvanic pairs; excepting that there is no insulation, all the plates being plunged in one vessel. I have usually separated the pairs by a board extending across the frame merely.

Indeed, when the forty plates were successively associated in pairs, of copper and zinc, though suspended in a fluid held in a common recipient without partitions, there was considerable intensity of Galvanic action. This shows that, independently of any power of conducting electricity, there is some movement in the solvent fluid which tends to carry forward the Galvanic principle from the copper to the zinc end of the series. I infer that electro-caloric is communicated in this case by circulation, and that in non-elastic fluids the same difficulty exists as to its retrocession from the positive to the negative end of the series, as is found in the downward passage of caloric through them.

It ought to be mentioned, that the connecting wire should be placed between the heterogeneous surfaces before their immersion, as the most intense ignition takes place immediately afterwards. If the connexion be made after the plates are immersed, the effect is much less powerful; and sometimes after two or three immersions the apparatus loses its power, though the action of the solvent should become in the interim much more violent. Without any change in the latter, after the plates have been for some time suspended in the air, they regain their efficacy. I had observed in a Galvanic pile of three hundred pairs of two inches square, a like consequence resulting from a simultaneous immersion of the whole*. The bars holding the plates were balanced by weights, as window sashes are, so that all the plates could be very quickly dipped. A platina wire, No. 18, was fused into a globule, while the evolution of potassium was demonstrated by a rose-coloured flame arising from some potash which had been placed between the poles. The heat however diminished in a few seconds, though the greater extrication of hydrogen from the plates indicated a more intense chemical action.

Agreeably to an observation of Dr. Patterson, electrical ex-

* See Plate III. fig. 3.

citement may be detected in the apparatus by the condensing electroscope; but this is no more than what Volta observed to be the consequence of the contact of heterogeneous metals.

The thinnest piece of charcoal intercepts the calorific agent, whatever it may be. In order to ascertain this, the inside of a hollow brass cylinder, having the internal diameter two inches, and the outside of another smaller cylinder of the same substance, were made conical and correspondent, so that the greater would contain the less, and leave an interstice of about one-sixteenth of an inch between them. This interstice was filled with wood, by plugging the larger cylinder with this material, and excavating the plug till it would permit the smaller brass cylinder to be driven in. The excavation and the fitting of the cylinders was performed accurately by means of a turning lathe. The wood in the interstice was then charred by exposing the whole covered by sand in a crucible to a red heat. The charcoal, notwithstanding the shrinkage consequent to the fire, was brought into complete contact with the inclosing metallic surfaces by pressing the interior cylinder further into the exterior one.

Thus prepared, the exterior cylinder being made to touch one of the Galvanic surfaces, and a wire brought from the other Galvanic surface into contact with the outside cylinder, was not affected in the least, though the slightest touch of the interior one caused ignition. The contact of the charcoal with the containing metals probably took place throughout a surface of four square inches, and the wire was not much more than the hundredth part of an inch thick; so that, unless it were to conduct electricity about forty thousand times better than the charcoal, it ought to have been heated, if the calorific influence of this apparatus result from electrical excitement.

I am led finally to suppose, that the contact of dissimilar metals, when subjected to the action of solvents, causes a movement in caloric as well as in the electric fluid, and that the phenomena of Galvanism, the unlimited evolution of heat by friction, the extrication of gaseous matter without the production of cold, might all be explained by supposing a combination between the fluids of heat and electricity. We find scarcely any two kinds of ponderable matter which do not exercise more or less affinity towards each other. Moreover, imponderable particles are supposed highly attractive of ponderable ones. Why then should we not infer the existence of similar affinities between imponderable particles reciprocally? That a peculiar combination between heat and light exists in the solar beams, is evident from their not imparting warmth to a lens through which they may pass, as do those of our culinary fires.

Under this view of the case, the action of the poles in Galvanic

vanic decomposition is one of complex affinity. The particles of compounds are attracted to the different wires agreeably to their susceptibilities to the positive and negative attraction; and the caloric, leaving the electric fluid with which it had been combined, unites with them at the moment that their electric state is neutralized.

As an exciting fluid, I have usually employed a solution of one part sulphuric acid and two parts muriate of soda with seventy of water; but, to my surprise, I have produced nearly a white heat by an alkaline solution barely sensible to the taste.

For the display of the heat effects, the addition of manganese, red lead, or the nitrats, is advantageous.

The rationale is obvious. The oxygen of these substances prevents the liberation of the gaseous hydrogen, which would carry off the caloric. Adding to diluted muriatic acid, while acting on zinc, enough red lead to prevent effervescence, the temperature rose from 70 to 110 Fahrenheit.

The power of the Calorimotor is much increased by having the communication between the different sheets formed by very large strips or masses of metal. Observing this, I rendered the sheets of copper shorter by half an inch, for a distance of four inches of their edges, where the communication was to be made between the zinc sheets; and, *vice versâ*, the zinc was made in the same way shorter than the copper sheets where these were to communicate with each other. The edges of the shortened sheets being defended by strips of wood, tin was cast on the intermediate protruding edges of the longer ones, so as to embrace a portion of each equal to about one quarter of an inch by four inches. On one side the tin was made to run completely across, connecting at the same time ten copper and ten zinc sheets. On the other side, there was an interstice of above a quarter of an inch left between the stratum of tin embracing the copper, and that embracing the zinc plates. On each of the approaching terminations of the connecting tin strata was soldered a kind of forceps, formed of a bent piece of sheet brass furnished with a screw for pressing the jaws together. The distance between the different forceps was about two inches. The advantage of a very close contact was made very evident by the action of the screws; the relaxation or increase of pressure on the connecting wire by turning them being productive of a correspondent change in the intensity of ignition.

It now remains to state, that by means of iron ignited in this apparatus, a fixed alkali may be decomposed extemporaneously. If a connecting iron wire, while in combustion, be touched by the hydrate of potash, the evolution of potassium is demonstrated by a rose-coloured flame. The alkali may be applied to the wire in

small pieces in a flat hook of sheet iron. But the best mode of application is by means of a tray made by doubling a slip of sheet iron at the ends, and leaving a receptacle in the centre, in which the potash may be placed covered with filings. This tray being substituted for the connecting wire, as soon as the immersion of the apparatus causes the metal to burn, the rose-coloured flame appears; and if the residuum left in the sheet iron be afterwards thrown into water, an effervescence sometimes ensues.

I have ascertained that an iron heated to combustion, by a blacksmith's forge fire, will cause the decomposition of the hydrat of potash.

The dimensions of the Calorimotor may be much reduced without proportionably diminishing the effect. I have one of sixty plates within a cubic foot, which burns off No. 16, iron wire. A good workman could get 120 plates of a foot square within a hollow cube of a size no larger.—But the inflammation of the hydrogen which gives so much splendour to the experiment, can only be exhibited advantageously on a large scale.

Explanation of the Plate (III).—A a, fig. 1st, two cubical vessels, 20 inches square, inside. b b b b a frame of wood containing 20 sheets of copper, and 20 sheets of zinc, alternating with each other, and about half an inch apart. T T t t masses of tin cast over the protruding edges of the sheets which are to communicate with each other. Fig. 2. represents the mode in which the junction between the various sheets and tin masses is effected. Between the letters z z, the zinc only is in contact with the tin masses. Between c c, the copper alone touches. It may be observed, that, at the back of the frame, ten sheets of copper between c c, and ten sheets of zinc between z z, are made to communicate by a common mass of tin extending the whole length of the frame, between T T: but in front, as in fig. 1, there is an interstice between the mass of tin connecting the ten copper sheets, and that connecting the ten zinc sheets. The screw forceps, appertaining to each of the tin masses, may be seen on either side of the interstice; and likewise a wire for ignition held between them. The application of the rope, pulley, and weights is obvious. The swivel at S permits the frame to be swung round and lowered into water in the vessel a, to wash off the acid, which, after immersion in the other vessel, might continue to act on the sheets, encrusting them with oxide. Between p p there is a wooden partition which is not necessary, though it may be beneficial.

Fig. 3. represents an apparatus alluded to p. 211.—It consists of a couronne des tasses, reduced to a form no less compact than that

that of the trough; hollow parallelepipeds of glass are substituted for tumblers or cells. The plates are suspended to bars counterpoised like window sashes.

The advantages are as follow: The material is one of the best non-conductors, is easily cleansed, and is the most impervious to solvents. The fracture of one of the cups is easily remedied by a supernumerary. They may be procured (as in the United States) where porcelain cannot be had. The shock from 300 pairs is such as few will take a second time: some of the effects have already been stated*.

XXXVI. *On Friction in Machinery.* By Mr. HENRY MEIKLE.

To Mr. Tilloch.

35, Berners Street, Aug. 27, 1819.

SIR,—As hinted at the conclusion of my miscellaneous remarks on friction, carriages, &c. in your Number for March, I again venture to resume that inexhaustible subject.

From the very accurate experiments of Professor Vince and M. Coulomb, it appears that, on hard bodies, friction is an uniformly retarding force; or, that it is constant during all the changes of an uniformly varied motion. This is, perhaps, the most valuable and general mode of experimenting, in order to discover the laws of friction, so far as the velocity is concerned; because, from it, we learn that friction is, *cæteris paribus*, the same for all the different uniform velocities which do not exceed the greatest attained in the experiment. We must not, however, like Chemists with their laws of expansion and temperature, suppose that friction is the same for velocities greater than those experimented on. Though this seems probable, it is not certain; and to presume on it, we might meet with the same downfall as the fanciful laws of temperature did when they attempted to soar above the ken of observation.

I formerly noticed that, on account of heat, friction sometimes increases a little with the velocity. This, the experiments referred to, were hardly calculated to detect, as it is not likely they were continued long enough to accumulate heat sufficient to affect the result. But there is, perhaps, some ground for suspecting that the friction of bodies which have long rubbed on each other, is somewhat different from the friction of those that only pass a few trials for the sake of experiment. I have often thought that the difference between the results obtained by Professor Vince and M. Coulomb, relating to the pressure, was probably owing to causes of this nature.

* The glasses may be had by applying to Edw. A. Pearson, No. 71, Cornhill, Boston.

In perusing Mr. Tredgold's papers in your Numbers for January and July, I was not a little surprised to find that he had arrived at the singular conclusion of the friction in uniform motions being *inversely* as the velocity—a doctrine directly opposed to by far the greater number of experiments I have ever heard of; which, with a very few exceptions, mostly lying the other way too, confirm the general opinion that, on hard bodies, friction is the same for all velocities. This led me to examine more closely the different steps of his investigation, in order, if possible, to find whence the mistake had originated. In the former of these papers, page 6, Mr. T. asserts that the depth of indentation is as the square of the time; and in July Number, page 23, he quotes the same, saying that "the depth of impression is as the force, and as the square of the time the force acts." Why he should have pitched on the *square* rather than any other power of the time, he has not thought proper to inform us. It may not, however, be altogether useless to show that it cannot be as the *square* of the time: it is well known, that when a heavy body meets with a resistance which still allows it to descend through a space proportional to the square of the time, that resistance must be *uniform*. In which case it is evident there could be no such thing as a maximum of indentation or impression; because the one body resting on the other, and meeting with no increase of resistance to impression, if it sunk at all, would continue to do so, with a uniformly accelerated motion, till it had got quite through the lower body—nor would it likely be at rest on this side of the earth's centre if time sufficient only were allowed.

Such are the ridiculous consequences to which this doctrine would lead. Nearly in the same way it might be shown that the depth of impression can be as no constant power of the time whatever. Indeed we have no data for determining the relation which subsists between the time and depth of indentation. But the resistance to indentation, without doubt, increases rapidly with the depth. This is abundantly evident from Mr. Rennie's interesting experiments.

In January Number, page 7, Mr. T. further states that "the space abraded will be as the square of the velocity." This I am not disposed to contradict, as he, no doubt, means the entire space abraded from the commencement of motion. But I confess I do not see how it applies: for, it seems clear that it is not the *whole* space passed over from the beginning, but only its *rate of increase* that he has to do with, and which must be as the *velocity simply*. There is just as good reason for including the whole space, from the commencement of motion, in the friction of the uniform motion, as there is in the accelerated. For,
however

however unknown the nature of friction may be, nothing seems more certain than that the friction, for any given instant, depends on the then value of, and relation which subsists at that instant among, the quantities concerned in producing it. *Present* friction has really no more concern with *past* space, than we have with "the years beyond the flood."

Besides the consequents of the mistakes already noticed, there are some other things in Mr. Tredgold's investigation, which, if not erroneous, are at least fanciful enough. But be these as they may, by combining *mollia cum duris*, he has luckily hit on the truth in his (10) prop., which, however, contains the condemnation of the (8).

The mistake first noticed about the indentation being as the square of the time, has no doubt given birth to his (8) prop. that—in uniform motions friction is inversely as the velocity; and this in its turn has been the fruitful parent of many strange inconsistencies. For instance, if friction were inversely as the velocity, it would necessarily follow that the thickness of an axle is a matter of total indifference as to the moving of the carriage: for, as the axle's diameter increases, the relative velocity of sliding at its surface, increases, and by prop. (8) *the friction diminishes*. But the effect of the power at the wheel's rim to turn it round, or overcome the friction at the axle, is also, *cæteris paribus*, inversely as the diameter of the axle. Consequently, the wheel must have the same disposition to turn, whatever be the axle's diameter. Let, therefore, the axle's diameter become nearly equal that of the wheel, which, if it turned before, will, for the same reason, turn still. The obvious conclusion then is, that on a polished railing it is in the wheel's option whether to turn or slide; or, that the use of wheels is merely to overcome the inequalities of the road. Mr. Tredgold has thus duly laid claim to the very delusion which I exposed towards the end of my former letter, though it be directly contrary to what he teaches us on page 21 of July Number. But the remaining pages of that paper are sadly infected by prop. (8)—the final conclusions, especially IV and VI, being quite contradictory. On these, however, I shall not now enlarge, but only remark, that it is indeed no small recommendation to any theory, however vague, that it be consistent with itself. This is at least a possible case; yet, how rarely do we meet with an instance of the kind!

Admitting that friction is constant for all velocities, it readily follows that friction, when referred to the first mover, or, in other words, that the waste of moving power in order to overcome friction, is, *cæteris paribus*, directly as the relative velocity of the rubbing surfaces. It is on this principle that friction wheels are

of any use, or that they even turn round : for, if friction were inversely as the velocity, they could (as we have already seen) have no disposition to move,—nay, their inertia would always oblige them to remain at rest.

Several other necessary observations I shall pass over at present, and conclude with one remark on Mr. T.'s statement already quoted, that (*cæt. par.*) “ the depth of impression is as the force.” Now this again obviously implies, that the resistance to impression is *uniform*, or the same at any depth, than which nothing can be more absurd ; since even on a fluid, the resistance increases exactly as the depth. How much more quickly must it do so on a hard road ! I am, sir,

Your most obedient servant,

HENRY MEIKLE.

XXXVII. *On poisonous Tea-Leaves.* By Mr. JAMES MILLAR.

To Mr. Tilloch.

SIR, — **T**HE regular establishments for the manufactory of imitation tea-leaves arrested not long ago the attention of the public; and the parties by whom these manufactories were conducted, together with numerous venders of the factitious tea, did not escape the hand of justice. The fraud of manufacturing sloe and white thorn leaves into an imitation of tea, which has been drunk by the public as the genuine beverage of tea, is comparatively harmless, when compared with the fraud lately detected of manufacturing real genuine unsaleable tea-dust into tea, by means of a process which renders the article absolutely deleterious to health. In proof of this statement, you will have the goodness to lay before the public, through the medium of your Magazine, the following facts.

A poor woman, having purchased an ounce of green tea, was struck by the lively blue colour which the beverage made of it assumed, on pouring into it a tea-spoonful of spirit of hartshorn. This person (a char-woman) being in the habit of frequently partaking of tea in other houses where she went to work, and being constantly in the habit of adding a tea-spoonful of hartshorn to the tea-beverage, without having observed that singular appearance which her own tea-leaves produced, made a complaint to the grocer from whose shop the tea was purchased. This person, unconscious of any deleterious admixture, having paid a fair price for his commodity, took a sample of the suspected tea-leaves to Mr. Accum the chemist, who analysed it, and pronounced it to contain copper. So unexpected a result induced
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the vender of the poisonous tea-leaves, whose whole support depended on the rectitude of a fair tradesman, to inquire into the fraud committed upon him. He consulted some of his friends who received their tea from the same quarter, and it became evident that the deceptions practised in this diabolical branch of commerce were greater than was by him expected. The poisonous tea had all the appearance of the leaves of genuine Hyson; but it was noticed by the chemist who examined the suspected samples, that a portion of the leaves when infused in boiling water became speedily resolved into a fine powder, and that part of this alone remained in an entire state, so as to make it possible to recognise the vegetable structure; and this led to the opinion that the manufacturer of the poisonous commodity had employed the dust of the leaves of Hyson tea, [the sale of which forms a regular business with many tea-brokers], and moulded it, probably by means of a small quantity of mucilage, into a compound possessing in every respect the external characters of genuine Hyson tea. This fraud may therefore be detected by merely throwing the sophisticated tea-leaves into warm water, which instantly causes them to fall into a fine powder, which speedily settles to the bottom of the vessel. The infusion, when mingled with liquid ammonia, affords a bright blueish green colour, indicating the presence of copper. But the presence of this metal may be more strikingly rendered obvious, by mixing two parts of the suspected tea-leaves with one of nitrate of potash (saltpetre), and throwing the mixture into a crucible kept red hot in a common fire. The whole of the vegetable matter of the tea will thus become destroyed, and the copper remain behind in combination with the alkali of the saltpetre. If water therefore be poured into the crucible to dissolve the mass, the presence of copper will be incontrovertibly rendered obvious, by the admixture of liquid ammonia, which imparts to the fluid a beautiful sapphire blue colour.

I am with respect, sir,

Your humble servant,

Grove Cottage, Lisson Green, Sept. 22, 1819.

JAMES MILLAR.

P. S.—Mr. Accum, in his Report, remarks that the copper employed for colouring the tea is in the state of a carbonate, and not as verdigris, which he states totally inapplicable for its fraudulent purpose of giving a bloom to the tea-leaves. I need not remind your readers, that all preparations of copper are deadly poisons.—J. M.

XXXVIII. *On the Geology of Loch Leven in Scotland.* By
Mr. GAVIN INGLIS.

To Mr. Tilloch.

DEAR SIR,—WHEN I sent you Outlines of the Geological History of Loch Lomond, I pledged myself to furnish a similar account of Loch Leven in Fife, or rather in Kinross-shire. Since that period I have been diligently making observations and collecting materials for that purpose.

Taking advantage of the present low state of the river flowing from the lake to examine the depth and strata of the alluvial soil, occupying what had formerly been deep water, and now forming the coarse lands between Auchmoor Bridge and Lochend, I took a trip up the river in a small flat-bottomed boat. I was favoured with a still day, a clear sky and radiant sun, whose rays darting direct to the bottom of the deepest pools, uninterrupted by the smallest ruffle on the surface, enabled me, by the reflected light, most distinctly to observe every layer of stratified soil, the roots, trunks and branches of large oaks lying at the depth, in some places, of 20 and 30 feet under the surface of the bank. Doubling on by the windings of the stream, where the last winter's floods had brought down part of the bank, I observed hanging loosely from the side, something like a piece of broken tile. Rowing immediately towards the spot, I discovered it to be a fragment of one of those ancient *terra cotta* vessels that have occasionally been dug up in different parts. Looking attentively into the opening left by the portion taken from the bank, I could observe the edge of remaining fragments, which I obtained by digging into the sandy clay; and from the position of what was thus procured, I judged the remainder must have fallen into the water with the avalanche from the bank, and might be found at the bottom. Shoving off the boat a little from the edge and remaining still, till the ruffle on the surface had subsided, I could distinctly observe the other parts lying at a considerable depth, part still adhering to a lump of clayey soil in which it had originally been imbedded. After many efforts and considerable difficulty, I succeeded in recovering all the mouth of the vessel, and some fragments of the side corresponding to those parts taken from the bank.

The mouth is $5\frac{1}{8}$ inches in diameter; no handles have been attached to it; and it is of this form: The whole bears evident marks of having been turned on a wheel. No glazing could be discovered either on the surface of the vessel itself, or in the surrounding soil, which was carefully examined on finding none attached to the surface.



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The position of the vessel, its contents, the soil in which it was imbedded, added to every other circumstance connected with its situation, leaves not a doubt on my mind, of its having found its resting-place by chance. It was buried two feet seven inches under the surface of the bank, with two unbroken *straths* of turf soil above it. Standing nearly erect, it was filled with the same materials in which it was enveloped. It must have been crushed by the consolidating pressure of the superstratum on the subsiding of the waters of the lake, as each fragment lay conjoined to its corresponding fracture.

Could the age of these vessels be ascertained, might not some data be formed as to the duration of this depth of the alluvial matter now occupying so large a proportion of what must have been deep water when the lake was in its primitive majesty? I intend sending these fragments to Edinburgh for preservation, and that they may be compared and remain with their kindred shreds of former times.

I am, &c.

Strathendry, Aug. 27, 1819.

GAVIN INGLIS.

XXXIX. *Notices respecting New Books.*

Peintures Antiques de Vases Grecs, de la Collection de Sir JOHN COGHILL, Bart. publiées par JAMES MILLINGEN, Rome, 1818.*

MR. MILLINGEN has been long honourably known as a learned and judicious antiquary; having published in 1812 a valuable collection of inedited Greek medals in quarto; and in 1813, a collection of paintings of Greek vases in folio, with 63 plates, preceded by an introduction full of curious researches and observations. This present work is the more valuable, as the collection of Sir John Coghill has been lately brought to the hammer, and we believe entirely dispersed. This precious collection had been formed by M. de Lalo, private treasurer to the late Queen of Naples; at his death it was purchased by Chevalier Rossi, and afterwards came into the hands of Sir John Coghill.

Mr. Millingen's present work contains in the introduction some positive ideas on the manufacture of the Greek vases of baked earth: they are in a great measure contained in three letters written by M. de Rossi.

The site of some very great ancient cities is still a subject of dispute and learned research; and two or three thousand vases of baked earth, most of them made four or five centuries before

* We are indebted for the following review of the ingenious work of our countryman Sir John Coghill, to the *Journal des Savans* for May last, and for the translation of it, to the Literary Gazette.

the vulgar æra, are the ornament of our cabinets. The dry and argillaceous matter of which they are formed, and the excavations in which they were contained, have ensured to them, as it were, an eternal duration; whereas, if we except Herculaneum and Pompeii, the oxidation caused by the humidity of the earth has every where destroyed almost all vases of bronze.

During the last century and a half, collections have been formed of painted Greek vases: endeavours have been made to guess at the processes employed by the potters: but time alone has revealed them to us. It was necessary to possess a very great number, to discover among them some which showed vestiges of the first labour; and to have eyes sufficiently exercised to be able to recognise and describe them; it is only within these few years that this could be done.

First a clay was chosen, in which baking would produce the colour desired, red, black, or yellow. When this clay was not to be had, they introduced into that which they were forced to employ, ochres (oxides of iron) to produce the colour. The vase thus formed was placed in the oven, where the first baking gave it so much consistency that the fluid colours would not sink in. From the hands of the potter these vases were transferred to those of the painter. The painter traced upon them, with a metal point, in dotted lines, the oval of the head and the parts of the limbs which were to be covered by the drapery; sometimes this sketch is done with a coloured line, but of a colour different from that of the ground: then he designed round this oval and the other dotted lines, with the pencil, a broad black line. This sketch and first operation are still visible on two vases in this collection, but they are generally hidden by the colours, which were laid on flat without degradation. Those vases are considered as the most ancient which are of only one colour, that of the clay of which they are made. Soon after they were painted black: hence comes the name of *Αἰθρες*, which Hesychius gives to the vases placed in the tombs; and the poor retained the use of them, while the rich employed those which were adorned with figures. The most common vases are still sought after on account of the beauty of the forms.

The vases adorned with painted figures are those which are the most highly valued. Antiquaries are agreed in considering as the most ancient, those of which the ground is yellow (the colour of box), and the animals painted on them, oftener than human figures, of a brick red. Mr. Dodwell found several in tombs near Corinth: the inscriptions painted upon some of them indicate the highest antiquity.

The vases of the second period have a yellow or white ground, and the figures are black; their inscriptions are for the most part
not

not to be read; the drawing is incorrect; the figures want life and expression; the subjects represented are frequently inexplicable, because the vases are anterior to the fourth century, when Zeuxis created, and caused to be generally adopted, constant modes of painting the gods and heroes. It is believed that the second sort of vases is that which was most generally imitated in the following ages, from love of archaism.

The ground of the vases of the third sort is black; the figures are yellow or red: these are the most common.

Sometimes we find on the Greek vases, blue, green, carmine, and even gilding. The white colour was added on the painting in the accessories, as well as the inscriptions; hence it happens that they have often been in part rubbed off. The white colour was partly made of white lead.

A second baking fixed the colours on the vases, and gave them that bright varnish which distinguishes the most precious of them. As for ordinary vases, a varnish was given to the whole ground before the baking, which in this case was not followed by a second.

The figures were generally copies, and not original, of the invention of the painters of the vases. M. de Rossi thinks we may infer this from the circumstance, that no painting is found in which the artist has corrected himself, that is to say, where he has departed from the dotted lines, or even changed any attitude.

Such are the mechanical details of the manufacture and painting; which was the part least known.

Very different judgements have been passed on the painters of the Greek vases of baked earth, or rather on the designers; for the name of painters should be reserved for those who create their subjects, and not mere copyists*. If we examine the variety, the elegance of the draperies, the beauty of the figures, the exactness of the proportions, we shall own that these designers have some merit, especially when we consider that the figures drawn on the convexity of the vases, and in the concavity of the pateræ, are in true perspective, so that they may not appear deformed: this is so true, that if we trace the outline of one of these figures, and lay it on a plane surface, it will seem to lean backwards, to fall (it is the art of the painter of ceilings). On the other hand, the extremities of these beautiful figures (the hands and feet) are drawn with as much negligence as we find in the

* Though we have in this place used the words *painters* and *designers* as in the French, it is not because we consider them as properly expressing the meaning intended, but rather because we could not fully make up our mind in the choice of two single words exactly corresponding. In fact, *designer*, from its root *design*, seems more strongly indicative of invention than *painter* does.—Edn.

pictures of savage nations. The designers (or drawers) were therefore not painters, properly so called; they were indifferent copyists: and the paintings on the Greek vases are not originals, but copies of pictures, bas-reliefs, or statues, which had acquired celebrity.

How could these designers make a collection of studies of these fine works? What substances did they employ in lieu of our different kinds of pencils, of our transparent paper? &c. It is probable that our mode of tracing was unknown to them, by which the most moderate artists trace faithful copies; what process did they use in its place? Perhaps they made sketches of the pictures which they intended to imitate, or of those which they had seen on their travels. Hence it comes that, in the paintings on vases, the principal parts are well executed, and the extremities are very incorrect. Having trusted the latter merely to their memory, they were incapable of drawing them faithfully. M. de Rossi illustrates this idea by a striking example. The potters of Urbino, the native town of Raphael, adorned their ware with subjects designed by that great master and his pupils; we recognise them by the nobleness of the style, by the spirit of the design; but how far is the execution from equalling the subject!

Notwithstanding the imperfections of the paintings on the Greek vases, the study of them must be very useful to our artists; they will find means to form their taste in the nobleness, the simplicity of the compositions, in the grace, the energy, the just expression of the attitudes. It is there we find true models of the folds of draperies, not only in figures at rest, such as statues, but also in figures that are in motion.

The name of *Sicilian vases* is improperly given to those in which the figures are distinguished from the ground by their black or dark colour, (whereas in the others, the figures are yellow on a dark ground,) and which are found in other places besides Sicily. This mode recalls to mind the invention of painting, the imitation of the shadow on a wall: the style of design is barbarous and incorrect; hence many antiquaries have assigned them the highest antiquity. They would have been in the right, if they had said that this style, appearing to be appropriated to masquerades, caricatures, and the like, was probably imitated at all times in subjects of this kind. What evidently proves it is, that in the manufacture of these vases, the elegance of their forms is the same as in the vases of the finest style, the vases of Nola: the same must be said of the ornaments which accompany the figures, flowers, festoons, &c., which are the same, and equally elegant. However, what we most frequently see on the vases called *Sicilian*, are Bacchanalia, that is to say, masquerades, orgies, caricatures,

catures, for which this kind of painting was perfectly adapted; and was probably affected to be retained for these subjects, from a spirit of religion. The Athenian coins afford a similar instance of affectation of archaism.

Since I have spoken of the festivals of Bacchus, I must speak also of the mysteries and initiations; because there has been established, since the time of Passeri and Montfaucon, an opinion, which ascribes all the painted vases to the initiated, whom they accompanied in the tombs. Mr. Millingen has successfully refuted this opinion. First, it is not founded on the authority of any ancient author. When we discover a collection of tombs, all containing vases more or less precious, will it be asserted that all the dead, whose remains are contained in these tombs, were initiated in the mysteries of Bacchus? What shall we say of those of children, who could not have been admitted to initiation, and which also contain vases?

For what reason were vases placed in, or near the tombs? The Greeks burned or interred the dead indifferently: as is proved by the vases containing bones and ashes, placed in some tombs, which are surrounded by other tombs, in which the dead are laid upon leaves. The first tombs contain fragments of vases, which were broken when they were thrown upon the funeral pile; those fragments were gathered up with the ashes, and bear evident marks of the action of fire. The vases improperly called *Lachrymatories*, which are found in the tombs, and the urns of the Romans, have the same origin. Their arms were interred with warriors, the appendages of the toilet with women; with both, the vases which had been valued; which had contained the wine, the oil, the milk, the perfumes used on the bodies, the central water which served for the purifications, the portion of the funeral repast which was consecrated to the dead, &c. Some placed these vases carefully in the tombs, others threw them in and broke them: hence the many fragments of vases, which the restorers artfully collect, filling up the vacancies with other pieces which they dexterously paint. (This fraud may be detected by applying acids to the newly painted parts.)

It is not my design to retrace the history of the discovery of the painted Greek vases, either at Corinth in the time of Julius Cæsar, or in Etruria and Campania, on the revival of learning, because these details are to be found in numerous works; nor to repeat the explanations of the paintings and of the inscriptions which Lanzi has judiciously restored for the most part to the Greek fables. I cannot, however, pass over in silence the explanation given by M. Akerblad, of the frequently repeated inscription *HOIATΣ KAAIOΣ*, which Mazzochi, Millin, &c. have read the
 Vol. 54. No. 257. Sept. 1819. P beautiful

beautiful Hopais. M. Akerblad reads *HO ΠΑΙΣ ΚΑΛΟΣ* (ὁ παῖς καλός) *the beautiful child.*

After giving just praise to the two collections of painted Greek vases by Mr. Millingen, I must speak of those published in France by M. Dubois de Maisonneuve. The first appeared in two volumes folio, with explanations by the late M. Millin under the title of *Peintures de Vases*. The public will doubtless receive with equal satisfaction the new collection publishing by the same gentleman, under the title of "Introduction à l'Étude des Vases Antiques d'Argile Peints," &c. and of which three numbers have been published out of the eight which the work is to contain.

An Inquiry into Doctor Gall's System concerning Innate Dispositions, the Physiology of the Brain, and Materialism, Fatalism and Moral Liberty, including some general Reflections on Prison Discipline, the Prevention of Crimes, and the Reformation of Malefactors, &c. By J. P. TUPPER, M. D. Fellow of the Royal College of Surgeons, F.L.S., Member of the Medical Society of Paris, and of the Society of Arts and Sciences of Bordeaux, and Surgeon Extraordinary to H. R. H. the Prince Regent*. Svo. pp. 150.

Dr. Tupper is of opinion that the doctrines of Dr. Gall and his co-professor Spurzheim† are wholly fallacious—that in their particular as well as their general application, they confound good and evil; and render it next to impossible to discriminate between virtue and vice, to distinguish innocence from guilt. The question is not one of that nature in the discussion of which we are disposed to participate. We feel somewhat under the same impression as Dr. T., when he remarks that the cranio-logical system in dispute "relates to things which appear far beyond the reach of all human understanding." p. 187. We owe it, however, to the author, to state that we have read his work with much pleasure; that we recognise fully the laudable motive which has induced him to venture into the field of philosophical polemics; and that, if he has not always succeeded in convincing us of the soundness of his views, he has at least left us most favourably impressed with his acuteness and candour as a controversialist.—The following are the subjects treated of in the course of the work:—Objections to the first principles of Dr. Gall's system.—Of innate dispositions.—Of the difference of

*Also author of *An Essay on the Probability of Sensation in Vegetables*, of which a second edition has been published, containing *Additional Observations on Instinct, Sensation, Irritability, &c.*

† We mention the name of Spurzheim, as he is the joint professor of the same philosophy, although we believe that there are a few points upon which they differ.

talents in different people.—Of dreams.—Dreams influenced by bodily sensations.—Of the renewal of the frame.—Of memory.—Influence of the will in our intellectual operations.—Nature of the soul incomprehensible.—Phænomena relating to the intellectual faculties.—Of instincts.—Of the instinctive organs according to Dr. Gall's system.—Of the analogy between reason and instinct.—Of the intellectual organs according to Dr. Gall's system.—Anatomical, physiological and pathological proofs against the same system.—Other objections.—Of materialism.—Of fatalism.—Of moral liberty.—Of the two-fold nature of man.—Influence of the organization.—Allusions to Scripture.—Of prison discipline.—Impropriety of assize-balls.—Of simple exile as a punishment.—Of death as a punishment.—Dr. Gall's system, a source of apologies for crimes.—Review of Dr. Gall's arguments respecting mental alienation.

The Elements of Natural Philosophy: Illustrated throughout by Experiments which may be performed without regular Apparatus. By JAMES MITCHELL, M. A. 8vo. pp. 362.

Although many treatises on a popular plan have been written on the Elements of Natural Philosophy, the experiments by which the principles of the science are illustrated, are, as the author of the present work justly remarks, in general such as can only be performed by means of a large assortment of philosophical instruments; and the individual who studies in private can only refer to the plates, and endeavour in his imagination to form an idea of what he reads. The work before us solicits distinction, on the ground that all the illustrations are drawn from the more ordinary phænomena of nature, from objects met with in common life, from experiments which may be performed with such things as a person in most circumstances may easily procure; and there can be no doubt that on such a ground there is room for establishing a very high claim to utility.

Mr. Mitchell's style of demonstration is in general sufficiently plain and perspicuous: if it wants any thing, it is a little more precision. The work aims in a particular manner at the instruction of the young; and nothing can be more likely to lead a philosophical tyro astray than a loose indication of essential principles. The experimental illustrations, on the strength of which the author rests his chief hopes of approbation, are uniformly of the simplest description, in many instances very apt, and, with a few exceptions, well calculated to impart clear ideas of the positions they are employed to elucidate and establish. We shall subjoin a few specimens extracted at random.

On Momentum.—"Some animals act instinctively as if they perfectly understood the art of increasing their momentum. A

ram, in fighting, butts with his hard forehead; but in order to increase his momentum he moves backward, and then coming forward with great velocity dashes his head against his antagonist. Making these animals fight, is a favourite amusement at the Court of the King of Persia. One of the Barber of Bagdad's brothers is said, in the Arabian Tales, to have been a favourite with the nobility for his skill in teaching these animals to fight. It is not improbable that the sight of rams assaulting each other, first suggested the idea of the warlike machine called the battering ram, which was used for breaking down the walls of a town." p. 39.

Centre of Gravity.—"In old buildings where the whole fabric is closely bound together, it may occur that a part may overhang the base, and yet that part not fall: but if the centre of gravity of the whole building were brought without the base, ruin would instantly ensue. The two towers of Bologna, in Italy, close beside one another, hang several feet beyond the perpendicular, and seem to beholders as if ready to fall; but as the whole building firmly adheres together, and as the centre of gravity is still above the base, they are perfectly secure. They must have been long in this state, as they are mentioned in the poems of Dante who died in 1586. The tower of Pisa is 182 feet high, and is swagged thirteen feet and a half from the perpendicular; it is built of fine marble, and is most firm and secure. No records can tell how long it has been in this state." p. 56.

The Pendulum.—"The same principle which occasions the motion of a pendulum, viz. that a body in its descent acquires force sufficient to raise it to the same height, has given rise to an amusement which is pretty common in Russia when the rivers are frozen over. The ice is piled up so as to form a declivity sloping with a smooth surface to the level of the river, and there commences another pile which rises to nearly the same height, but not quite. It again slopes down to the river, and again another commences, and so on. A person gets into a vessel like a butcher's tray, and gliding along the first pile of ice acquires a velocity which carries him up to the top of the second, down which he goes and ascends the third, and so on. In summer they employ wood instead of ice. Near Paris, in the summer of 1816 this Russian amusement was introduced, and gave great satisfaction to the Parisians, and it still continues in favour." p. 102.

Hydrostatics.—"The rapidity with which water flows from a hole in the side of a cask is in the same proportion as above stated (as the square root of the depth). Get a bucket or cask and make a hole in the lower part of it, and mark at the side, by means of any scale or measure, the height of one inch, of four inches, of nine inches, of sixteen inches, and of twenty-five inches.

inches. Fill the bucket or cask as high as twenty-five inches, and notice exactly, when the water is running out, how long time it takes to sink as low as sixteen inches ; it will require exactly the same time to sink from sixteen to nine inches ; also exactly the same time to sink from nine inches to four inches ; the same time also to sink from four inches to one inch ; and then the same space of time for the remaining part of the water to run entirely out. It was upon this principle that the *Clepsydra* (or Roman water-clock) was founded. Cæsar mentions making use of them as measures of time, in his expedition into Britain." p. 151.

From an unceasing endeavour to be extremely simple, it is not surprising that the author should in some instances have been betrayed into offences against the dignity of science. The following illustrations we feel disposed to rank among this number. "A man of greater weight striking a less man has an advantage." "A ship at sea running against a boat, or a small vessel, will *probably* upset it or dash it to pieces." "A stone on the ground will not leave its place, except some one remove it." "If a bird hovering in the air were not drawn down, it might continue for ever to do so." "Point a gun in a certain direction, and the shot flies that way." "If a piece of wood be thrown into the Thames when the wind blows right across, the wood will be carried to the other side, but lower down."

On the whole, however, the work is one which deserves to be recommended to the attention of the philosophical student, but more particularly of those who may be desirous of acquiring a respectable stock of scientific information without encountering the difficulties and fatigues inseparable from a more systematic course of study.

XXXIX. *Intelligence and Miscellaneous Articles.*

DANGER OF CLEANING WINE-BOTTLES BY MEANS OF SHOT.

IT is well known that bottles in which wine has been kept, are usually cleaned by means of shot, which by its rolling motion detaches the tartrate of potash from the sides of the bottles. This practice, which is generally pursued by wine-merchants, may give rise to serious consequences, as will become evident from the following case :

A gentleman who had never in his life experienced a day's illness, and who was constantly in the habit of drinking half a bottle of Madeira wine after his dinner, was taken ill, three hours after dinner, with a severe pain in the stomach and violent bowel colic, which gradually yielded within twelve hours to the re-

medies prescribed by his medical adviser. The day following he drank the remainder of the same bottle of wine which was left the preceding day, and within two hours afterwards he was again seized with the most violent colliquative pains, head-ach, shiverings, and great pain over the whole body. His apothecary becoming suspicious that the wine he had drunk might be the cause of the disease, ordered the bottle from which the wine had been decanted, to be brought to him, with a view that he might examine the dregs, if any were left. The bottle happening to slip out of the hand of the servant, disclosed a row of shot wedged forcibly into the angular bent up circumference of it. On examining the beads of shot, they crumbled into dust, the outer crust (defended by a coat of black lead with which the shot is glazed) being alone left unacted on, whilst the remainder of the metal was dissolved. The wine, therefore, had become contaminated with *lead and arsenic*, the shot being a compound of these metals, which no doubt had produced the mischief.

PERUVIAN BARK.

In the *Journal de Pharmacie* for May last, there is a curious detail of effects produced by an atmosphere impregnated with *cinchona*. M. Delpech of Guayra (the port of the Caraccas) had stored up (in 1806) a large quantity of newly collected *cinchona*—filling several apartments on the ground-floor. Being visited by a number of friends, he was obliged to put some of them in the rooms occupied by the *cinchona* (each containing from eight to ten thousand pounds). These apartments were of much higher temperature than the rest of the house, occasioned by the fermentation of the bark. A bed in one of them was occupied by a traveller ill of a malignant fever (then very prevalent). He found himself much better after the first day, though he had taken no medicine; and in a few days he was perfectly restored. This unexpected event induced M. Delpech to place other persons ill of fever in his magazine, all of whom were speedily cured, simply by the effluvia of the bark.

M. Delpech had deposited along with the bark a bale of coffee selected for his own use, and some bottles of French brandy, all of which remained for some months in the midst of the *cinchona*. After this time, M. Delpech on visiting his magazine observed one of the bottles uncorked; and suspecting a servant had been making free with it, he determined to try the quality of the brandy. He was much astonished to find its quality greatly improved; having acquired somewhat of an aromatic flavour, and become more tonic and agreeable. This improvement he could only attribute to the bottle having been left uncorked; for on opening the others they were found no way altered: but being
then

then left open, they soon acquired all the good qualities of the first bottle.

The bale of coffee was now opened, and a portion of it was roasted. Its flavour was found much altered: it was more bitter, and left in the mouth a taste similar to an infusion of bark.

The bark which produced these effects was fresh. Would the cinchona of commerce produce the same effects? This question can be answered only by experiment.

EXTRAORDINARY CURE OF LOCK-JAW.

[From the Quebec Gazette.]

The interest of suffering humanity tending at times to excite public attention, I publish the following case, which even the gentlemen of the faculty will not peruse with indifference. It is well known with how little success the medicinal art has hitherto struggled with that terrible disorder known by the name of Tetanus (*Opisthotonos*), especially when caused by a wound.

On the 15th of December last, Mary Saint Gelais, 19 years old, a servant to Mr. Saul, fell on the glazed frost and lacerated the integuments of her right knee; but the wound not appearing dangerous, she continued her usual occupations. Eighteen days after, although the wound appeared perfectly healed, she began to complain of a stiffness in the back of her neck, and a certain difficulty in moving her jaw; accompanied with a pain in her knee, which the curing of the wound had not been able to dispel. The pain having increased in an alarming manner during the day, the patient was carried in the evening to Dr. Blanchet, who prescribed something for the night—Dr. Iffland being called in, at Mr. Saul's desire, declared that the tetanus was then complete. During three days he employed in a masterly manner every thing that the art prescribes in such cases; but perceiving all his efforts were useless, he requested his friend Dr. P. De Salles Latterriere to form a consultation. Of this, the result was amputation; to which, nevertheless, the patient and her relations positively refused their consent. They then contented themselves with enlarging the wound, and dressing it with the common stimulants, leaving the patient with such full conviction of her approaching death, that they thought it their duty to give her warning of her extreme danger, and her relations of the certainty of her death.

I found the poor woman in so violent a paroxysm that her whole body was bent like a bow, and supported only on the back of the head and on the heels. The jaws were so closed that it was impossible to introduce the blade of a knife. I confess that I also thought her on the very point of expiring; yet her pulse, although weak and rapid, and much resembling such a one as

commonly accompanies the inflammations of the brain, holding out tolerably well, I immediately came to the resolution of bleeding her until she fainted. I was obliged to take from her thirty-six ounces of blood—The fainting fit lasted a long time; but the contraction of the jaws and the general spasm yielded visibly to that powerful depletion—I then took advantage of the slackness of the jaws to make her swallow four ounces of castor oil, and I prescribed the same quantity in a clyster;—after two hours she had two copious stools. She notwithstanding relapsed, and as violently as before; I repeated the bleeding, which was followed by a fainting after a fresh loss of eighteen ounces of blood.

During the three following days she took each day an ounce and a half of good laudanum;—the fourth day her mouth again closed, and the same convulsions began: another bleeding, *ad deliquium*, thirty ounces; and the patient found herself relieved as it were by enchantment. Her great repugnance to the tincture of opium made me substitute in its place the extract of pure opium combined with calomel. The doses will appear more than extraordinary, and the success alone can justify them: I gave her three days successively, sixty grains of opium alone; the calomel did not cause any salivation; it acted powerfully on the bowels, from which it expelled several worms of an astonishing length. The woman is at present perfectly cured of the tetanus, though extremely weak, which weakness her excessive poverty will in all probability keep up but too long.

Quebec, February 1, 1819.

JOS. PAINCHAUD.

BISMUTH.

It appears by experiments made by M. Chaudet, that bismuth, even when covered with charcoal, is entirely volatilized in a temperature of 30° Wedgwood, if exposed to this heat for a sufficient length of time.—*Ann. de Chim. et de Phys.* ix. 397.

TIMBER.

From recent researches made in Sweden, it appears that the birch reaches the furthest north, growing beyond the 70th degree; the pine reaches to the 69th; the fir to the 68th; the osier, willow, aspen, and quince, to the 66th; the cherry- and apple-tree to the 63d; the oak to the 60th; and the beech to the 57th: while the lime, ash, elm, poplar, and walnut, are to be found only in Scania.

NATURAL HISTORY.

The splendid collection of zoology, lately purchased from Dufresne of Paris, for the College Museum, Edinburgh, has reached its destination in safety. It consists of 1600 birds; 12,000 insects; 2000 shells; 800 eggs of different species of birds; besides corals, quadrupeds, and amphibious animals.

VACCINATION.

VACCINATION.

Between the years 1752 and 1762 the small-pox carried off in Copenhagen 2644 persons; in the next ten years 2116;—from 1772 to 1782 the victims to this disease were 2233;—and from 1782 to 1792, 2735. In 1802 vaccination was introduced and enforced by authority; and from that time to 1818 only 158 persons were carried off by small-pox; viz. in 1802, 73; in 1803, 5; in 1804, 13; in 1805, 5; in 1806, 5; in 1807, 2; in 1808, 46; in 1809, 5; in 1810, 4;—since which time not a single case of small-pox has occurred in the dominions of the King of Denmark. After such a fact, can it be longer doubted that the small-pox, which has carried off infinitely more victims than all the wars and pestilences which ever afflicted the human race, may be entirely extirpated from the face of the earth? This being once effected, vaccination itself would become unnecessary.

PORTABLE GAS LIGHTS.

Mr. Gordon of Edinburgh has taken out a patent for this contrivance, which consists in condensing from 20 to 30 atmospheres of the gas in a vessel of sufficient strength, and furnished with one or more apertures for combustion, with proper stop-cocks. A globe of one foot diameter properly charged with gas, will yield a light equal to six common candles for twelve hours; and so in proportion for other sizes. The forms of course may be varied.—The result of this contrivance will be, that families will by and by send their servants to the *gas-maker* (as formerly to the candle-maker) to get their portable magazine charged and ready for lighting every day, or every second day, without subjecting themselves to the trouble of making the gas in their own houses.

THE SYMPIESOMETER.

This name is given by Mr. Adie of Edinburgh to a new instrument of his invention, for indicating those minute changes in the weight of the atmosphere which might be supposed to arise from the action of the sun and moon. Its indications are given by the pressure of the atmosphere. He employs an elastic fluid or gas (hydrogen is best), and any liquid (excepting quicksilver) not liable to be acted on by the gas which it confines, nor by the air, to a contact with which it is in some measure exposed. The liquid he prefers is almond oil coloured with anchusa root. The whole is inclosed in a tube with double bulbs, and fitted to a common thermometer.

NEW HYGROMETER.

This instrument, the invention of Mr. Adie, is composed of a small bag made of the internal membrane of the *arundo phragmites*,

mites, and fitted, like a bulb, to the lower end of a thermometer tube. It is then filled with mercury, which rises and falls in the tube, by the sensible and rapid changes that take place in the contraction or dilatation of the membrane, from the humidity or dryness of the atmosphere. In point of sensibility, Mr. Adie has found this membrane to exceed any thing he has ever met with.

LITHOGRAPHIC PROCESS.

[Concluded from p. 157.]

Lithographic chalk is a composition which can be applied to the stone in a dry state like Italian chalk; and different sorts of drawings may be produced with it. For the composition of this chalk M. Senefelder gives a number of recipes. We extract the first. Wax 4 parts; soap 6 parts; lamp-black 2 parts. The wax and the soap are melted together, then the lamp-black is added; the whole is well rubbed down on a hot iron plate, then put into a saucepan and exposed to the fire until it returns to a liquid state. It is then poured out on a stone plate, well impregnated with oil, so as to form a cake of the thickness of the eighth part of an inch. The mass when cooled is cut into small slices, and is fit for use. In the other compositions, tallow, spermaceti, and shell lac are added in various proportions, as it is required to make the chalk of a harder or of a softer texture.

There remains the composition of the printing ink, which should be made of oil varnish, and fine burned lamp-black, well ground and united. Sometimes ivory-black is used, and occasionally Frankfort black.

The next thing considered by M. Senefelder, is what is called "the preparation of the stone;" by which is especially meant the process by which the stone receives the quality of repelling the printing ink in certain distinct places. Gum arabic, and some other similar substances, are the principal means of thus preparing the stone; the operation of aquafortis or other acids only rendering the stone more disposed to admit of the preparation. As the gum acts only on the uppermost surface of the stone, and by the frequent wiping off of the ink in printing is gradually diminished, it becomes necessary to renew it, while the stone is printing, about twice a-day. M. Senefelder's work contains a mass of minute and valuable information on this point.

The best mode of making a steel pen for lithographic purposes is then described; as also the manner of cutting sable pencils in order to qualify them for the production of lines of equal thickness. The lithographic student must also provide himself with tracing and etching needles; and with a chest well lined with pitch, in which to bite in the stones.

There are three different sorts of paper necessary. Transpa-
rent

rent paper ; blotting paper ; and printing paper. The transparent paper is for the tracing of drawings, in order to transfer them to the stone ; which may be done either in the ordinary way, by rubbing the back of the drawing with black-lead, and, having laid it on the stone, marking the outlines with a tracing needle, or by making the drawing on the paper with the softer chemical ink already described, and, having laid it on the stone, passing it through a press ; in which case the paper must undergo a particular preparation ; and is then called prepared transfer paper. The blotting paper is used principally as a covering for the paper which is to be printed. With regard to the printing paper, the best is the half-sized, or wholly unsized paper used for copper-plate printing. It ought to be moderately wetted, and then compressed, and left for twenty-four hours before it is used.

The description given by M. Senefelder of the various kinds of lithographic presses is not susceptible of abridgement, and would be unintelligible without the plates. He acknowledges that this is a part of the art capable of great improvement ; for that at present too much is trusted to the skill and attention of the printer.

We proceed to the account of the different manners of lithography. They are divided into two principal branches—the elevated, and the engraved manner. In the first, all those parts of the stone that are covered by a greasy ink, resist the action of the acid poured over the whole surface of the stone, by means of which the other parts of the surface become corroded ; they stand therefore higher than the latter, as if elevated from the plain surface of the stone. In the second manner, all those lines or parts of the drawing or writing which are to give the impression are engraved into the surface of the stone by means of a sharp needle, or bitten into it by the action of an acid.

The sub-divisions of the elevated manner, are 1st, the pen, or hairbrush drawing ; 2d, the chalk manner ; 3d, the transfer manner ; 4th, the wood-cut manner ; 5th, the scraped manner ; and 6th, the sprinkled manner.

With reference to the pen or hairbrush drawing, the stone, in order to prevent the chemical ink from spreading, ought to be slightly prepared, by washing it with a strong solution of soap and water, and subsequently with pure water, which ought to be carefully wiped off. The drawing is then to be made on the stone with the chemical ink already described. When dry, the biting in with acid may commence. This is effected either by a flat varnishing brush, or by effusion. In the first case, a composition of aquafortis, and three or four parts of water is repeatedly and uniformly passed over the surface of the stone ; in the second case, the stone having been previously placed in the
pitched

pitched chest before noticed, a composition of aquafortis, and 20, 30, or 40 parts of water is poured over it. Experience alone can teach the exact strength of the acid, or the proper duration of its action. The stone having been properly bitten in, should be washed with water; and, when dry, covered with a solution of gum arabic in four or five parts of water. In two or three minutes, a few drops of water and of oil of turpentine should be spread over the surface of the stone; and then, by means of a woollen rag, the whole of the drawing may be wiped off. The surface of the stone should then be well wiped with a damp rag, so that it may be every where slightly wet, and immediately charged with printing ink by passing a printing roller several times over it. The stone may then be printed; to simplify which operation, M. Senefelder gives very copious instructions; to which we must refer our readers.

The lithographic chalk is used on the stone in the same manner as common black chalk is used to produce a drawing on paper; a coarser or smoother grain having first been communicated to the stone by rubbing it with the finest gravel sand, and a little soap water. The chalk will not however bear so much corroding as the ink. In general therefore, for that purpose, to one part of aquafortis, a hundred parts of water may be added; and the darker shades may be further bit in by means of the flat varnishing brush and stronger acid. The printing of a chalk drawing on stone is extremely difficult, and requires strict attention to a variety of cautions recommended by M. Senefelder; who likewise explains at considerable length the way in which defects or accidents may be remedied; and describes a mode of producing a very agreeable effect by using two plates; on the one of which, a sort of middle tint is bitten in all over, with the exception of the high lights of the subject, and in the other the shades are expressed. Several stones may thus be brought into play; and a variety of colours thereby introduced.

The transfer manner M. Senefelder considers as the most important part of his invention. The paper for this manner must be previously prepared with a thin starch, mixed up with French chalk, plaster of Paris, and gamboge. The drawing or writing is effected on it with a dilution of the chemical ink in soft water. When dry, the back of the paper must be sponged with very weak aquafortis until it is thoroughly soaked, when the superfluous moisture must be absorbed by blotting paper, and being placed with its face on a clean stone, the whole must be passed two or three times through a press. When taken out, it must be put into the pitched chest, and a solution of aquafortis in water (one part of aquafortis to a hundred parts of water) be poured over it so as to wet the whole surface. Pure water must then be
poured

poured over it in like manner until the paper is disengaged. The solution of gum may then be applied, and the stone is ready for printing; although its effect may be increased by a rebiting. If chalk be used instead of ink, it must be previously softened by the addition of a little tallow.

We have no room for a description of what M. Senefelder calls the wood-cut manner, the sprinkled manner, and the Indian ink manner; or for an account of his mode of printing in colours, and in gold and silver. Leaving the elevated, we must content ourselves with a very brief notice of the engraved manner of lithography.

The stone, when it is intended to engrave upon it, must be rubbed down as smooth as possible, and prepared with gum water, which, however, must be immediately washed off. It should then be covered by a flat varnishing brush with a thin colour, composed of a solution of gum and lamp-black or red chalk. When perfectly dry, the drawing must be either traced or sketched on the surface; and the lines must then be all drawn in with an etching needle, cutting through the covering coat, and entering more or less deeply into the stone as a greater or less degree of strength of shade is required. A soft ink, composed of thin varnish, tallow, and lamp-black, must then be rubbed over the surface of the stone, into all the lines; and immediately wiped off, together with the original covering coat, by means of a woollen rag dipped in gum-water.

A drawing may also be etched on stone by a process similar to that used in etching on copper.

Some very curious and diversified processes of lithography are also described by M. Senefelder, under the names of—Manner of drawing with prepared or gum ink, the sprinkled aquatint manner, the soft ground manner, &c. all of which exhibit very striking ingenuity, and deserve to be closely studied by those who are desirous of obtaining proficiency in this new art. Instructions are also given for printing with oil and water colours at the same time, for the application of the stone to calico-printing, for printing oil-paintings by transfer, &c. We regret that although M. Senefelder's recently invented stone paper, intended to supersede the use of stone, is again mentioned, the mode of preparing it is not specified; M. Senefelder, however, promises to make this the subject of a separate work.

POMPEII.

In prosecuting the excavations at Pompeii, they have lately discovered several edifices in the fine street that leads to the temples of Isis and Hercules, and to the Theatre. In a house supposed to have belonged to some man of science, some surgical instruments were found of excellent workmanship; also some paintings representing fruit and animals, executed with great truth.

THE COMET.

A late conjecture that on the 26th of June the earth was in the direction of the tail of the comet now visible, is fully confirmed, since the orbit has become better known. The sun, the comet, and the earth, were on the 18th of June in the morning so nearly in a right line, that the comet was to be seen on the sun's disk. According to calculation, the nucleus of the comet entered the sun's southern limb at 5^h 22^m A.M. true time at Bremen. It was nearest to the centre of the sun 1' 27" west, about 7^h 13^m, and issued from the sun's northern limb about 9^h 22^m. The comet during this most remarkable transit was something more than seven millions of German miles distant from the sun, and about fourteen millions of miles from the earth.

Bremen, July 28, 1819.

W. OLBERS.

LECTURES.

The Courses of Lectures of the St. George's Hanover-Square, on Physic, Chemistry, and Surgery, will commence on Monday, October 4th, 1819.

The Medical Lectures, by George Pearson, M.D. F.R.S. &c.

The Chemical, by W. T. Brande, Esq. Professor to the Royal Institution, and Secretary of the Royal Society.

The Surgical, by B. C. Brodie, Assistant Surgeon to St. George's Hospital.

The *gratuitous* Lectures of Sir Everard Home as usual, to the Pupils of St. George's Hospital.

LIST OF PATENTS FOR NEW INVENTIONS.

To James Head, of Lower Brook-street, Grosvenor-square, esq. for a machine or instrument for ascertaining the difference of ships' draught of water forward and aft at sea or in harbour.—27th of July 1819.

To Henry Tritton, of Clapham, for an improved apparatus for filtration.—11th of August.

To Charles Phillips, of Haverford West, commander in the Royal Navy, for certain improvements on capstans.—20th Sept.

To William Brockedon, of Poland-street, for certain improvements in wire-drawing.—20th Sept.

The celebrated Mr. Watt of Birmingham died at his house at Heathfield, on Wednesday the 15th of September. Mr. Watt was elected a Fellow of the Royal Society of Edinburgh in 1784; of the Royal Society of London in 1785; and a Member of the Batavian Society in 1787; in 1806 the honorary degree of Doctor of Laws was conferred upon him by the spontaneous and unanimous vote of the Senate of the University of Glasgow; and in 1808 he was elected a Member of the National Institute of France.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1819.	Age of the Moon.	Thermo- meter.	Baro- meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
Aug. 15	24	76.	29.66	Cloudy
16	25	72.	29.75	Ditto
17	26	73.	29.80	Ditto
18	27	70.	29.92	Ditto
19	28	69.	29.88	Ditto
20	new	68.5	29.90	Ditto
21	1	67.	29.88	Ditto
22	2	72.	29.88	Ditto
23	3	76.5	29.70	Fine
24	4	78.	29.60	Ditto
25	5	73.	29.50	Ditto
26	6	68.	29.70	Cloudy
27	7	72.	29.70	Ditto
28	8	74.	29.55	Fine
29	9	73.	29.23	Ditto—brisk wind.
30	10	69.5	28.80	Showery
31	11	61.	28.96	Stormy
Sept. 1	12	58.	29.24	Ditto
2	13	61.	29.40	Cloudy—rain at night.
3	14	70.	29.23	Fine
4	full	69.	29.50	Ditto
5	16	66.	29.40	Ditto—heavy rain this morning.
6	17	62.5	29.60	Ditto
7	18	75.	29.94	Ditto
8	19	76.	29.64	Cloudy
9	20	74.	29.70	Ditto
10	21	74.	29.59	Ditto
11	22	65.	29.80	Ditto
12	23	66.5	29.98	Fine
13	24	66.5	29.98	Ditto
14	25	69.	29.90	Ditto

METEOROLOGICAL TABLE;
 BY MR. CARY, OF THE STRAND,
 For September 1819.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock Morning.	Noon.	11 o'Clock Night.			
Aug. 27	64	73	60	30.15	56	Cloudy
28	63	71	61	29.97	47	Cloudy
29	60	71	60	.75	62	Fair
30	59	61	58	.45	56	Stormy
31	56	60	51	.55	61	Fair
Sept. 1	54	64	51	.70	66	Fair
2	53	65	61	.79	70	Fair
3	64	72	60	.81	45	Fair
4	66	70	66	.96	47	Fair
5	64	65	55	.92	22	Rain
6	56	66	51	30.09	61	Fair
7	64	70	66	.14	21	Cloudy
8	67	74	66	.22	51	Fair
9	65	74	63	.22	51	Fair
10	63	72	61	.15	40	Fair
11	56	63	60	.20	16	Cloudy
12	58	67	54	.33	54	Fair
13	55	67	54	.36	55	Fair
14	56	71	61	.30	61	Fair
15	60	71	62	29.95	30	Cloudy
16	56	57	43	.84	37	Fair with rain at
17	53	62	55	30.14	58	[night]
18	61	67	56	.31	45	Fair
19	55	60	48	.27	55	Fair
20	49	61	47	.42	62	Fair
21	52	60	54	.35	49	Fair
22	50	60	46	.49	47	Fair
23	51	62	53	.26	40	Cloudy
24	56	64	53	29.95	46	Fair
25	56	62	52	.43	0	Rain
26	53	59	52	.56	36	Showery

N.B. The Barometer's height is taken at one o'clock.

ERRATUM.

Page 112, line 12, *for* orbit; likewise since *read* orbit likewise since,
 116, — 29, *for* Cocanada *read* Cocavada.

XLI. *Remarks on Mr. MEIKLE's Paper on finding the Longitude by Lunar Observations.* By Mr. EDWARD RIDDLE.

To Mr. Tilloch.

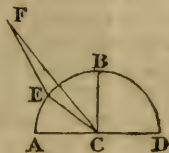
SIR, — **W**HEN a person who undertakes to animadvert upon the views of others, executes his purpose with moderation and candour, all that the public is concerned in knowing is, whether his objections are well founded. But when, in the plenitude of self-satisfaction, such a person in every paragraph of his inquiry affects to hold in derision the talents of all who have previously considered the subject; it is necessary not only that the justness of his particular objections should be quite indisputable, but that the correctness of his belief in the superiority of his own acquirements should either be apparent, or admit of being easily proved.

The immediate cause of these remarks is a miscellaneous letter on the subject of lunars, &c., by Mr. H. Meikle, printed in your Magazine for July. A perusal of that letter will show even those who may be unacquainted with the subject of it, the spirit in which it is written.

What confidence may be put in the author's assertions, what weight attached to his judgement, or what regard ought to be paid to his censures, I purpose at present to inquire.

He censures in severe terms the deduction which, in low altitudes, is sometimes made from the semidiameters of the sun and moon, before they are applied to the observed distance. He says, "we shall afterwards see that this is an elaborate way of creating new errors." As I do not afterwards see, in Mr. M.'s letter, a satisfactory reason to believe any such thing, I shall show, before I proceed further, that in the principle of the correction no error exists.

Conceive ABD to represent the upper half of the moon's disk, and let us suppose that it is an ellipse whose horizontal and vertical semidiameters are AC and BC . Suppose also that F is a star, and FE its measured distance from the nearest point E of the moon's disk. Even when $\frac{BC}{AC}$ has the



least value which, in the practice of lunars, it can ever acquire from the effect of refraction; and when, from the position of F , the angle $FE C$ is also a minimum; and further, when FE is the least distance that is used in the solution of the problem;—

$FE + EC$ will differ from FC by an almost unappreciable fraction of a second. If it were possible for FE to be $175''$, which it never can be; and if FE were 15° , the difference between $FE + EC$ and FC would not exceed $\frac{1}{1000}$ part of a second. It would, in fact, be too minute for computation with the best tables in common use. In all cases, therefore, $FE + EC$ may be taken for FC , without sensible error.

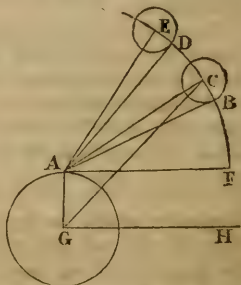
Now CE is necessarily less than AC , from the property of the curve; the theoretical existence of the correction is therefore apparent, notwithstanding Mr. M.'s censure of those whom he affects to call the "learned authors" that have recommended it to be applied.

He has indeed charged those authors with a mistake which he must be content to have attributed to himself. It is not, as he says, the line DE (in his figure) which they "have been pleased to consider as shortened by refraction;" but the line drawn from the *centre* to the point in the disk from which the distance is measured. And it has just been shown that if the semidiameters so reduced be applied to the observed distance, the result will differ from the *apparent central distance* by a quantity indefinitely small. I proceed, in the next place, to show that the method which has hitherto been pursued by those computists whom Mr. M. describes as "aiming at great exactness," will always give *correctly* the true and apparent altitudes of the centres of the objects.

Setting aside the effect of refraction, the centre of the luminary will be in the line joining the observer and the centre of the disk. And if, from the effect of refraction, the centre of the disk be elevated, the centre of the luminary, having the same altitude, will be elevated in the same degree. The apparent place of the centre, therefore, will still be in the line joining the observer and the point to which the centre of the disk is elevated by refraction.

Let then B, C , be the true places of the lower limb, and the centre of the luminary; and D, E , the apparent places of the same points. BAC will represent the true or augmented semidiameter, and DAE the reduced or contracted semidiameter corresponding to the altitude FAD .

If now to DAF there be added the reduced semidiameter DAE , the sum will be the angle FAE , obviously the apparent altitude of



of the centre E above the sensible horizon AF. If the refraction EAC corresponding to FAE be deducted from that angle, the remainder will be the angle CAF, the true altitude of the centre F above the sensible horizon; and if the parallax ACG corresponding to CAF be added to it, the sum will be HGC, the true altitude of the centre above the rational horizon GH.

This is the principle of the method which is generally adopted in correcting the altitudes; and it obviously gives correctly the true and apparent altitudes of the centre of the luminary. Therefore by the methods on which Mr. M. animadvert with so much severity, the true and apparent altitudes of the *centres* of the objects will be *correctly* obtained; and the *apparent distance of the centres* will also be obtained *correctly to within a very small fraction of a second*. The distance may then, from these data, be cleared from the effects of parallax and refraction, by the usual formula; and from whatever points the apparent distance is estimated, the result of the computation will be the true distance of the same.

Now the object of the computation for clearing the distance, is to deduce, from observations made upon the surface of the earth, the angular distance of the objects observed as they would be seen at the centre of it. By comparing the distances so deduced with those previously computed for a known meridian, the time at that meridian is found. And as those previously computed distances are the distances of the *centres* of the objects, it is obvious that the distances compared with them ought to be the distances of the same points.

Mr. Meikle proposes as an improvement, that the apparent distance should be estimated not from the centre, but from an ex-centric and variable point!

What I have already said will enable us to appreciate the value of this extraordinary proposal, the absurdity of which is only to be equalled by the air of self-gratulation with which it is delivered, and the contemptuous allusion which the author of it makes to "the old-established habit" of estimating the apparent distance from the centre. It would indeed appear that he is not aware that the *central distance* of the objects is what is required.

I have now, I trust, sufficiently vindicated the deduction from the semidiameters applied to the distance from the charge of a tendency to produce error. I have also shown that, in the correction of the altitudes, the deduction from the vertical semidiameters ought to be applied to obtain *correctly* the altitudes of the points of which it is the object of the problem to compute the distance. In the principles, therefore, which direct the common application of the corrections in question, there is no error.

Those which Mr. M. imagines he has detected are merely consequences of his own misconception.

But after all his affectation of superior precision, it will not perhaps be expected that the improved method which he proposes for finding the true altitude is founded on the same principles, and must always produce exactly the same result, as that of which he has endeavoured to prove the fallacy.—Yet that this is the case will be easily made appear.

From the apparent altitude of the limb FAD (see last figure) he deducts the corresponding refraction, which is DAB; to the remainder he adds the true semidiameter BAC, and the sum is obviously the angle FAC, the true altitude of the centre above the sensible horizon, and of the same magnitude as we have before determined it to be.

If further elucidation be necessary, the truth of the matter, in any given case, may be put to the vulgar test of arithmetical computation. Let us take an extreme case, and suppose the altitude of the lower limb to be 5° , and the semidiameter parallel to the horizon $16'$.—The computation for the true altitudes above the sensible horizon by both methods will stand as under :

By the common Method.

Observed altitude, lower limb	..	$5^{\circ} 0' 0''$
Reduced semidiameter		$0 15 35$
Apparent altitude centre		$5 15 35$
Refraction to $5^{\circ} 15\frac{1}{2}'$		$0 9 28$
True alt. above sensible horizon	..	$5 6 7$

By Mr. MEIKLE'S Method.

		$5^{\circ} 0' 0''$
Refraction to 5°		$0 9 53$
True alt. lower limb above sens. hor.		$4 50 7$
Horizontal semidiameter		$0 16 0$
		$5 6 7$

This I hope will be satisfactory.

On this part of the subject it appears then, that if Mr. M. has produced nothing new, he has proposed nothing which will lead to error. But when to the angle FAD he directs the “augmented” semidiameter, that is the angle BAC, to be applied, and considers the result as the apparent altitude; he immediately gets perplexed in what he calls the angular point of *the triangle*,
and

and determines the altitude of a point which, as we have already seen, has nothing to do with the computation. The difference between the true and apparent altitudes, technically called the *correction* of the altitudes, which is a most important circumstance to be attended to, will in this way be erroneous by the whole effect of refraction in contracting the vertical semidiameters.

I have now shown the reason which has hitherto induced astronomers to follow “the old established method” of estimating the apparent distance from the apparent place of the centre; and that there is nothing erroneous in the principle of making a deduction from the horizontal semidiameters before they are applied to the observed distance.—I have also shown that the centre is the point from which the distance must be estimated; and that if the reduced semidiameters be applied to the observed distance of the limbs, no appreciable error can arise from considering the result as the apparent distance of the centres. And I have lastly shown, that as the semidiameters and other corrections are usually applied by careful computers, in correcting the altitudes, the true and apparent altitudes of the centres are correctly determined. The distance computed from these data may therefore be depended on as correct in theory.

It is obvious also that Mr. M.’s proposal of applying an *augmentation* to the semidiameter applied to the distance, instead of making a *deduction* from it—and of taking an excentric point in the disk for the angular point of the triangle—is founded on misconception, and must produce error. Such is the consequence that would result from the adoption of Mr. M.’s theoretical views.

I shall now inquire whether any error would arise worth notice in the practice of lunars at sea, from neglecting some of the corrections of which I have shown the existence in theory. It is obvious that if any small correction be omitted which ought to be directly applied to the apparent distance, the true distance will be erroneous by nearly the same quantity. The deduction from the semidiameter applied to the distance is therefore a correction which cannot be dispensed with where great accuracy is required; though when the altitudes are above 15° the correction becomes so minute, that in observations taken at sea it will generally bear but a small proportion to the unavoidable errors of observation.

With respect to altitudes for correcting lunar distances, it may be observed generally, that if the apparent central distance be correctly obtained, no conceivable mistake in the application of the semidiameters in correcting the altitudes, can entail a mistake worth regarding on the result of the computation.—If the apparent altitudes be determined to a minute or two, the dif-

ference between the apparent and true altitudes may generally be determined to a second; and under such circumstances the resulting distance will not be affected to an extent worth caring for by any such small errors in the altitudes themselves.

But if the case were otherwise, such errors could not always be avoided in observations taken at sea. For the observations which have been made with Dr. Wollaston's Dip-sector show that from the variableness of the horizontal refraction, the apparent depression of the horizon will sometimes differ more than three minutes from its mean quantity.

In the apparent altitudes, therefore, greater errors are generally unavoidable than the imaginary ones which drew from Mr. Meikle this querulous exclamation—"It is in vain that we expect accuracy even from the best observers and instruments, if such needless errors are persevered in."

It is however evidently pushing the calculation to a nicety inconsistent with the data, to compute the apparent altitude of the centre to a second, when that of the limb may possibly be affected by an error of two or three minutes; and in altitudes for clearing the distance, it is fortunate that such precision is not necessary.

In the apparent distance we have seen that much greater accuracy is requisite; and it is one of the advantages of the lunar method of finding the longitude, that this element, the distance on the correctness of which the precision of the result so materially depends, can generally be measured with very considerable exactness.

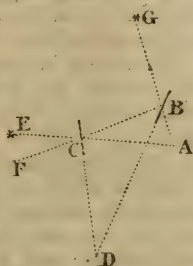
But supposing the elements determined from observation to be correctly obtained, it is certainly desirable that we should not in any degree vitiate the result of a good observation by mistakes in the theory on which our computation from it is founded. What the theoretical principles are which must guide us in the preparatory steps of the operation, I hope the reader of the preceding remarks will not need to be informed. And if he have already been instructed in the method of finding the longitude by lunar observations "according to the old established method," it may afford him some satisfaction to perceive that the cavils which Mr. Meikle has raised against that method are entirely without foundation.

Dismissing this subject, I come to his remarks on the principles of the quadrant. Though it is true that if a ray of light be reflected from a revolving mirror, the angle described by the reflected ray will be double that described by the mirror; and though it is an obvious consequence of this property that the angle measured by the revolution of the index in the quadrant is half that described by the reflected image of the object observed;

yet,

yet, as the instrument is constructed, the principle of its operation may be explained, and in fact is generally explained, from the elementary property of which this one is a consequence; viz. "the angle of reflexion is equal to the angle of incidence." With the assistance of this elementary property of reflexion, the principle of the instrument is generally demonstrated from the geometrical properties of the figures formed by the rays and the planes of the mirrors, at the time at which an observation is conceived to be completed. The angle to be measured, and that by which its magnitude is inferred from the instrument, are thus exhibited and directly compared.

Let *B* represent the index mirror, and *C* the horizon glass, and let the planes of those mirrors be produced to meet in *D*. Let *G* be a distant object, *GB* a ray from it falling on the index mirror, and reflected from it to the horizon glass *C*; and reflected there again to the eye at *A*, the point at which the reflected ray would meet the direct one *GB* produced.



To an observer at *A*, the reflected image of *G* would appear at *E* a point in *AC*; and the angle *GAE* would measure the angular distance of the object and its reflected image; or of the object and any thing with which the reflected image appeared to coincide. Now from the elementary optical property already referred to, *ABC* is bisected by *BD*; and *ACF* formed by *AC* and *BC* produced is bisected by *CD*. Whence $\angle CDB = \angle FCD - \angle CBD = \frac{1}{2} \angle FCA - \frac{1}{2} \angle ABC = \frac{1}{2} \angle ABC + \angle BAC - \frac{1}{2} \angle ABC = \frac{1}{2} \angle BAC$. But *BDC*, the inclination of the mirrors, is equal to the angle measured by the revolution of the index; and that angle is therefore half that which it is required to determine.

With a view to this method of investigating the principle of the instrument, as it is actually constructed and used, there is therefore nothing vague or insignificant in the description which those authors have given, who have stated in substance that "in consequence of the double reflexion, the angle measured by the instrument is doubled to obtain the measure of the required angle."—From the first reflexion the angle *ABC* is bisected, and from the second the angle *ACF*; and it is in consequence of these two reflexions that the angle *BAC* is formed in the instrument.

The caution with which Mr. Meikle represents the writers on navigation to have followed "the old beaten track," will not probably be diminished by the success of his "wanderings in search of improvement."

By the term parallax, astronomers understand the angular distance between the places of the centre of a celestial body, as seen at the surface, and at the centre of the earth; or, which amounts to the same thing, the angle under which the earth's semidiameter would appear when viewed from the centre of the celestial object. This angular distance is the arch of a vertical circle, and consequently, to an observer on the earth, parallax, *in changing the apparent places of objects*, operates only in a vertical direction.

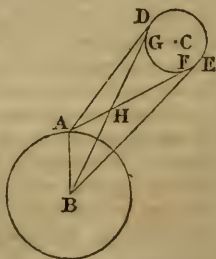
The "common popular doctrine" to which Mr. M. alludes, is therefore a true doctrine; and little more is necessary to be said on the subject when it is evident he misunderstands the term.

But though in changing the apparent places of objects parallax operates only in a vertical direction, its effects on the apparent distances of objects from each other are estimated in every direction. If Mr. M. were elevated as high above London bridge, as in his own estimation he is elevated above the insignificant crowd of writers on navigation,—though the effect of that elevation on his distance from surrounding objects would be apparent in every direction, the elevation itself would be estimated in a vertical one; and it would be in that direction chiefly that he would be solicitous to protect himself against the consequences of a fall.

When parallax is understood in the sense in which it has been here explained, it obviously cannot, by operating only in a vertical direction, have any such effect on the figure of the moon as Mr. M. affirms it would have. And though he says it would be very easy, he cannot show that the apparent figure of a spherical body would be at all affected by parallax operating only in a vertical direction.

But even taking the meaning of the term in the unusual sense in which he appears to understand it, it is not quite true, as he says in the next sentence, that "the difference of the parallaxes of any two diametrically opposite limbs constitutes what is called the augmentation of the diameter."

To prove this, conceive the earth and moon to be cut by a plane through their centres, B and C. Let A be a point in the great circle which bounds the section of the earth, and from A and B let the tangents AD, AF, BG, BE, be drawn to the circle which bounds the section of the moon. GBE is the measure of the moon's diameter at the centre of the earth, and DAF its measure at the point A; and $DAF - GBE$ is the augmenta-



tion.

tion. Now if the points G, D, and, also F, E, coincided, the angles at those points would represent the parallaxes for the two opposite limbs; and in that case $DAE + ADB = DHE = AEB + EBD$. Whence $AEB - ADB = DAE - EBD$. But the points G and D, F and E, are necessarily different points; and the angles formed by AD and BG produced, and by BE and AF produced, would not be the parallaxes of points upon the surface of the moon. The error indeed will be inconsiderable, but still it is an error; and the augmentation can easily be computed on principles to which no theoretical exception can be taken.

Dr. Mackay concludes, that the change of parallax at any altitude answering to a change of altitude equal to the moon's semidiameter will be the augmentation answering to that altitude. This is theoretically true. Mr. Meikle perhaps imagines that it is the same principle as that which he has delivered.

He next animadverts on the method of finding the latitude by a table of "difference of altitude of the pole-star and pole," and he declares that it "claims strong reprobation."—"It is always erroneous," he says, "except the latitude be nearly 0, or when the star is in the meridian."

The authors who have inserted that table in their works, knew, I hope, as well as Mr. Meikle, that the latitude found by it was only approximate; and they probably knew also that under circumstances in which the altitude of the pole-star could be observed at sea, the maximum error of the approximation was too insignificant to be regarded. In high latitudes where the error becomes worth notice, the pole-star cannot be seen during summer; and ships do not frequent very high latitudes in winter.

The "liberality" of the supposition that "the altitude of the pole-star when six hours distant from the meridian is equal to that of the pole," is not to be attributed to ignorance. It is assumed as a hypothesis nearly true, and from which a useful practical rule may be deduced. By the aid of that table, a common watch and a quadrant, an observer but indifferently skilled in computation may ascertain his latitude in a favourable night, as often as he pleases, and to a degree of exactness sufficient for all practical purposes at sea. It is on this account only that the table is given; not because the latitude determined on the hypothesis on which the table is constructed, is strictly correct.

He remarks further that no provision is made for the effect of "the rapid change of polar distance to which the star is subject." This remark is not generally correct. In Nerie's Epitome, any error of this kind is guarded against in his remarks on the use of the table. And in the valuable tables of Mendoza Rios, where the same method is given in a slightly different form,
any

any such error is also provided against by directing the polar distance to be taken out for the time of observation.

Mr. Meikle lastly mentions a table of this kind, in which the star's polar distance at the time of the publication of the table is erroneous by 4.6'. The table is in Mackay's Navigation, and the error is about what Mr. M. has stated it to be.

But in disingenuously endeavouring to convert this circumstance into an argument against putting confidence in any tables of the kind, he betrays an aberration from rectitude of intention, which shows that he is not more apt to suspect others, than to give, in his own conduct, cause for suspicion.

I have now gone through the whole of this gentleman's objections, and at greater length than I originally proposed to myself; and I hope I have made it apparent that the "errors and delusions" which he has so liberally, so confidently, and so unceremoniously ascribed to the writers on navigation, have no existence but in his own imagination.

Your obedient servant,

EDWARD RIDDLE.

P. S.—This does not appear an improper occasion to make a few remarks on some observations in page 31st of the pamphlet which Captain Ross has published in reply to that of Captain Sabine.

Speaking of Captain S. and himself, he says, "but, as we differed on the method of applying the refraction *minus* parallax, I made no use of his observations—he judging it proper to correct the observed altitude of the sun's limb for the semidiameter, before he took out the refraction; and I, being accustomed to apply that correction to the altitude of the sun's limb, before the semidiameter was applied. In the *Alexander*, printed forms were found according to the former method, which having confirmed Captain Sabine in his first opinion, he continued in this error during the voyage, and therefore neither his latitudes, longitudes, or variation were correct; and whenever I had occasion to make use of his altitudes, I took them as corrected only for the index error and the dip of the horizon." And he adds in a note, "the printed forms for the present expedition have been altered in consequence."

There is in this statement an apparent inconsistency—Captain Ross says in one place that, for a certain reason, he made *no use* of Captain S.'s *observations*; and in another place he says, "when I had occasion to use his" (Captain S.'s) "*altitudes*, I took them," &c. It is not, however, on this circumstance that I wish to remark.

It appears from what I have said in the foregoing letter, that if Captain S. applied the contracted semidiameter to the observed altitude, and then the refraction, &c. corresponding to the altitude of the centre so found, both the true and apparent altitudes of the centre would be obtained; the true altitude in fact would be the same as that obtained in the way in which Captain Ross says he has been accustomed to apply the corrections. And it appears also, that even if the semidiameters which Captain S. applied were not corrected for the effect of refraction, the circumstance was of little or no importance in lunars, if the corrections were taken out for the approximate altitude of the centre which the semidiameters so applied would produce. In problems indeed where the *true altitude only* was wanted, the deduction from the semidiameter ought to have been made; and if Captain S. did make it, Captain Ross's objections are unfounded; as the method was then only in appearance different from his own.—If Captain S. did not in such cases make the requisite deduction, his results would be in theory slightly incorrect. But if Captain Ross has been accustomed to consider the correction for parallax and refraction corresponding to the altitude of the limb, as the deviation in altitude which ought to be used in the computation for clearing the distance, I need not here tell those who may honour the preceding letter with a perusal, that, considered even in the most unfavourable light, Captain S.'s method of correcting the altitudes must produce results much nearer the truth than that of Captain Ross. No considerable mistake *in the longitude* can have arisen from the way in which Captain S. is said to have applied the corrections. But if I understand Captain Ross rightly, this cannot be said for the method of applying them in the forms which have been furnished for the present expedition, or for the similar method which he says he has been accustomed to pursue.

But before forming any conclusion on the subject, it appears desirable that more precise information should be afforded than that which is contained in the above extract from Captain Ross's pamphlet.

If both the forms which were used by Captain Ross and Captain Sabine in the late voyage were laid before the public; and if the work, at length, of an observation as it was taken, and worked, by each gentleman, during the voyage; and further, if the printed forms which were found on board the *Alexander*, and those which have been furnished for the new expedition, were given,—we should determine with more certainty the merits or defects of either method, than we are likely to do from any account which we may have of them otherwise.

If any of your correspondents could furnish you with these documents, I feel assured that you would most readily make room for their insertion in the pages of your Magazine.

XLII. *An Essay on Dreaming, including Conjectures on the proximate Cause of Sleep.* By ANDREW CARMICHAEL, M.R.I.A.*

DREAMS have perplexed every individual who has attempted to account for them; but it will scarcely be credited that a philosopher of the eighteenth century, who was acquainted with the opinions of Locke and had controverted with ability the theory of Berkeley, should find no other mode of explaining these phenomena, than by maintaining that "our dreams are prompted by separate immaterial beings †;" and in illustration of the nature of uneasy dreams during illness, could argue that "these beings wait for and catch the opportunity of the indisposition of the body, to represent at the same time something terrifying also to the mind ‡."

But to arrive at a more rational explication, we must revert to this author's predecessors. "Dreaming (says Locke) is the having of ideas (whilst the outward senses are stopped, so that they receive not outward objects with their usual quickness) in the mind, not suggested by any external objects or known occasion, nor under any choice or conduct of the understanding at all §;" and again, "this I would willingly be satisfied in, whether the soul when it thinks thus apart, and as it were separate from the *body*, acts *less rationally* than when conjointly with it, or no? If its separate thoughts be less rational, then these men must say that the soul owes the perfection of *rational* thinking to the *body*: if it does not, it is a wonder that our dreams should be for the most part so frivolous and irrational, and that the soul should retain none of its more rational soliloquies and meditations ||." We might almost imagine that this passage was composed in support of the Organic Theory since developed by Gall. Some feeble anticipation of such a system seems to have been floating in the mind of Locke.

* From the Transactions of the King's and Queen's College of Physicians in Ireland.

† An Inquiry into the Nature of the human Soul, wherein the Immateriality of the Soul is evinced from the Principles of Reason and Philosophy. Anon.—no date—p. 215.

‡ Id. 257. I have lately found that this is the first edition of Baxter's celebrated Essay.

§ Locke's Essay, 21st edition, vol. 1, p. 213.

|| Id. p. 87.

Hartley seems also but little inclined to attribute these phenomena alone to the soul. His views, as far as they go, are clear and satisfactory. "Dreams (he says) are nothing more but the imaginations, fancies or reveries of a sleeping man; and they are deducible from the three following causes: 1st, The impressions and ideas lately received, and particularly those of the preceding day. 2d, The state of the *body*, particularly the *stomach* and *brain*. 3d, Association*." "The scenes which present themselves are taken to be real, we do not consider them as the work of the fancy; but suppose ourselves present and actually seeing and hearing what passes. Now this happens, because we have no other *reality* to oppose to the ideas which offer themselves: whereas, in the common fictions of the fancy, while we are awake, there is always a set of real external objects striking some of our senses, and precluding a like mistake there: or, if we come quite inattentive to external objects, the reverie does so far put on the nature of a dream as to appear a reality†."

Beattie appears to have entertained a very confused idea of the nature of the soul, and even to have conceived that some of her faculties fall asleep while others remain awake. He does not, however, exclude the influence altogether of the corporeal organs. "The imagination or fancy (he remarks) seems to be almost the only one of our mental powers which is never suspended in its operations by sleep; of the other faculties, some are more and others less affected, and some appear to be for a time wholly extinguished‡." "Persons (he continues) who think much and take little bodily exercise, will, perhaps, be found to be the greatest dreamers; especially, if their imagination be active, and *their nervous system* very delicate§."

Darwin's doctrine is also connected with corporeal relations, as may be found in his observation, that the "perpetual flow of the trains of ideas which constitute our dreams, and which are caused by painful or pleasurable sensation, might at first view be conceived to be an useless expenditure of sensorial power||." We at least learn that, in his opinion, they really occur during an accumulation of this power, when he adds, that "our dreams in the morning have greater variety and vivacity, as our sensibility increases, than at night when we first lie down¶." But this hypothesis is not less vague, though apparently more philosophic, than the exploded systems of the nervous fluid and animal spirits. The sensorial power is in fact the nervous fluid stripped of its substance and reality, and reduced to a quality or attribute.

* Hartley on Man, quarto, p. 226.

† Id. p. 227.

‡ Beattie's Dissertations, moral and critical, vol. 1, p. 272.

§ Id. p. 274.

|| *Zoonomia*, London 1801, vol. 1, p. 287.

¶ Id. p. 302.

Professor Stewart's explanation is altogether psychological, but, however ingenious, is not even satisfactory to himself. He maintains "that all our mental operations which are independent of our will, may continue during sleep; and that the phænomena of dreaming may perhaps be produced by these, diversified in their apparent effects in consequence of the suspension of our voluntary powers *." "That the same laws of association, which regulate the train of our thoughts while we are awake, continue to operate during sleep; but the influence of the will being suspended, all our voluntary operations, such as recollection, reasoning, &c. must also be suspended †."

To this hypothesis he tells us that Mr. Thomas Browne and Mr. Prevost offered one and the same objection, viz. That unless the will were active, there could be no effort of attention, and without such an effort, there could be no recollection: yet we recollect our dreams, although the hypothesis supposes that in sleep the will does not operate. Professor Stewart expresses himself sensible of the force of this objection, and acknowledges that he is far from being satisfied that it is in his power to reconcile completely the apparent inconsistency. He, however, adopts a solution offered by Mr. Prevost, viz. that in perfect sleep there is no recollection; and that when we remember our dreams, our sleep has not been perfect. And he adds, that in bodily indisposition, the disturbed state of our rest may prevent the total cessation of the power of attention, which may enable us afterwards to retrace our dreams, or some accidental association may renew the train of ideas; and if we are satisfied that they once passed through our mind, yet not during our waking moments, we have no other alternative but to regard them as a dream †.

This last observation can scarcely be controverted. On awaking in the morning, we naturally advert to the occupations which are to employ us during the day, and the persons with whom we wish or expect to communicate. The moment they occur in our reflections, we perhaps recollect that we have been dreaming of them. But surely this is only an argument that our sleeping thoughts are recalled, like our waking, by the association of our ideas. It neither proves nor disproves his hypothesis. If the influence of the will, and the exercise of attention, may be suspended during the course of those thoughts which pass through our mind while asleep, and which yet may be recalled by the force of association, so may they be suspended also during our waking thoughts, and with a similar result. But, if they be necessary to our recollection in one case, they must in the other.

* Philosophy of the human Mind, octavo, p. 333.

† Id. ib.

‡ Id. note O.

With respect to the former solution of the difficulty, viz. that the disturbed state of our rest may prevent the total cessation of the power of attention, or, in other words, that we are not quite asleep during dreams, (which by the way appears the most prevalent opinion entertained at present on the subject,) it is only necessary to consider the difference between sleeping and waking, and reflect on our daily experience, to subvert the position. So long as *we sleep*, we are, with some singular exceptions, either totally insensible, or sensible only of internal sensations. In the transition from sleeping to waking, we are perhaps passively and involuntarily sensible of external impressions. And when these force themselves on our attention, or when we can voluntarily attend to them, from that moment we are fully *awake*. Nor are we conscious of being awake until we perceive those impressions, whether by compulsion, or with our will. Sometimes we notice the very moment this occurrence takes place; but in general, if we are not suddenly awakened, we perceive our sleep gradually retiring—we are reluctant to resign its embraces; we cling to it as long as we can; thoughts pass in crowded trains through our minds; yet these thoughts are not dreams. They assume no bodily shape or visionary semblance—we regard them as we do the current of our waking reflections, we direct their course as we please, and we know that we can break through the dubious remnant of our sleep, the moment that we will. It is otherwise when we actually sleep. The generality of our thoughts assume a corporeal appearance, and pass in fantastic procession before us. If our dream be terrific, we struggle to escape from the object of horror, and that struggle awakes us. Once to a certain extent awake, we banish with an effort the little residue of sleep; that effort is an act of the will; but whether our previous visionary struggle can also be ascribed to its power, is a question too involved for me to disentangle.

Yet I am tempted to venture a reflection which, upon the principles of the *new* philosophy, may lead to a solution of the difficulty. If a single organ be awake, and a single motive be presented to it, it acts in obedience to that motive without choice, without judgement, without a decision, and, of course, without any intervention of the will. But if two motives offer themselves to the faculty in question, and it chooses between them, or if two organs are awake, and judge between the claims of two opposite motives, one applicable to this, and the other to that propensity or sentiment, and that a decision is the result, whether to act or to forbear, to pursue or to fly, this decision, though in a dream, appears to be an act of the will. It would be called so without hesitation, if these operations happened to take place during

during our waking moments. Are we then to conclude, in opposition to so respectable an opinion as that of Professor Stewart, that our voluntary powers are not always suspended when we dream?

But these are not the difficulties which appear to me to weigh most against his hypothesis.

In supposing that the influence of the will is suspended, it also supposes, as this revered philosopher expresses himself, that all our voluntary operations, such as recollection, reasoning, &c. must also be suspended.—If there be in nature pure mental operations, recollection and reasoning are entitled to the rank; yet we learn from this hypothesis that a simple essence, such as the soul is considered, can be at the same moment, with respect to its different powers, awake and asleep—all its operations which are independent of the will may continue, but its recollection and reasoning must be absolutely suspended. Dr. Beattie's explanation involves the same incongruity. All the faculties, according to him, are more or less affected, and some for a time wholly extinguished, while the operations of imagination or fancy are alone unsuspended by sleep. The elucidations of Hartley are exempt from this inconsistency, and remove every difficulty but one—why sometimes we dream, and sometimes do not. Darwin repeats an exploded hypothesis in a more plausible form. Locke simply discusses the phenomena of dreaming in proof of his position, that men think not always; he does not attempt to account for them; yet from the questions detailed in the passage I have quoted above, it is manifest that he was inclined to refer the explanation to organic rather than to spiritual operation.

If we but do so likewise, all these difficulties vanish; we are not driven to the absurdity of supposing that sleep is necessary to a pure spirit, and that its simple essence may be half asleep and half awake—a moiety of its powers suspended or extinguished, and another moiety active and busy. We may look for the solution in the corporeal organs of the soul, and not in the soul itself; there is nothing incongruous in supposing that some of these organs may be in a state of activity while others are at rest. “Watching (says Dr. Spurzheim) is called the state wherein the will can put in action the organs of the intellectual faculties, of the five senses, and of voluntary motion; but it is impossible to define watching as the state wherein all these organs are active, for it cannot happen that all the faculties should be active at the same time; all organs, being fatigued, take rest, and this state of rest is sleep; but any particular organ, or even several organs, may be active while the other organs rest; then the peculiar sensations or ideas which result from this particular activity constitute

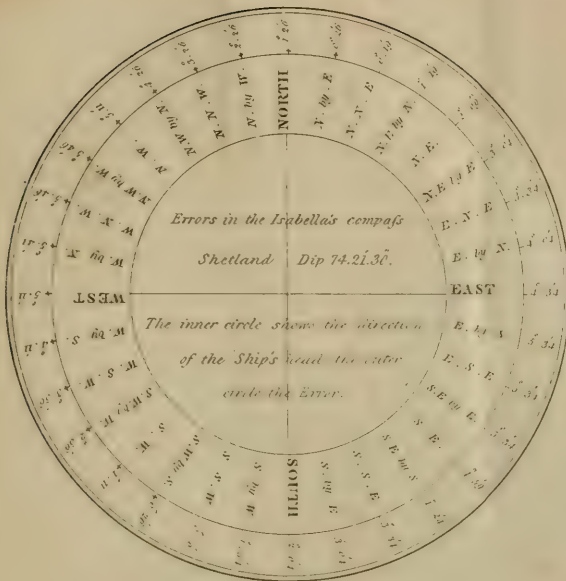
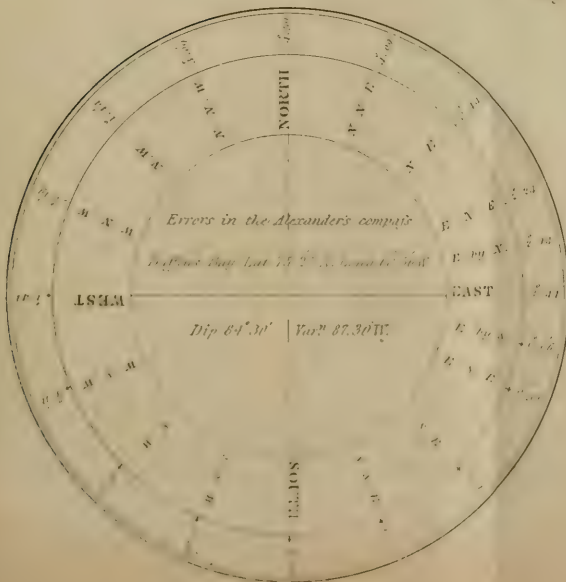


Fig. 3



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constitute that which is called *dreams*, which are more or less complicated according to the number of the active organs*.”

This, on a comparison with the preceding opinions, appears to be a probable and satisfactory explanation, as far as it goes ; but our curiosity requires a more detailed elucidation of the nature of sleep. It may properly be said, that it is rest after fatigue ; but we know that it is something more. We can rest when it is necessary, without that intense and predominant change which locks up our senses and intellect, and envelops us with inadvertence and oblivion of the past, present, and future. We cannot reflect on the nature of this state, without being satisfied that it involves some important vital process, so indispensable as to be of daily recurrence, and of such general influence as to engage every part of the frame, but particularly the organs of thinking, sensation, and voluntary motion. If we ask ourselves what process is of prime necessity to those organs, we can answer without difficulty, *that* which repairs their waste, and preserves their consistence and vigour—the process of assimilation. Whatever may be the result of its operation in the bones and muscles and other coarser parts of the body, we can scarcely reflect on its action upon the delicate texture of the brain and nerves, without perceiving that it must be accompanied by powerful and overwhelming effects. These are the fragile instruments of thought, feeling, and motion ; and no wonder that a change which affects their very structure should be attended with a cessation of their functions, and the actual *paralysis* of sleep.

Yet this is but a conjecture ; and so obscure and inaccessible is the subject, that, however we may wish for certainty, we must be contented with mere plausibility. There is no decisive fact to support the position ; and, like first principles, if its own reasonableness does not carry with it conviction, it must, for the present, remain destitute of proof. Still we ought not to disregard any phenomenon that may lead to clearer views of the subject. Most animals sleep shortly after their meals ; and there are few climates in which men do not allow themselves the same indulgence. In these countries, this is not so generally the practice ; and it may be a question, whether it is not by an effort that we at first acquire a habit not natural to us, and overcome one which we originally found to be almost irresistible, and to which, perhaps, we should be compelled to submit, if we were not able to interrupt or suspend the process of assimilation in the nervous system. It is true that we can have no direct influence over this process in the grosser parts of the frame ; but our power may be more extensive in the seat of the intellect. By an effort we can

* Spurzheim's Physiognomy, 1st edition, p. 216.

continue to think—by thinking we *exercise* the organ of thought—by exercising the organ we may possibly interrupt or defer this process, whose invasion, when effectual, subdues every faculty of the mind. Young children are destitute of this power—they sleep almost incessantly; but nutrition and assimilation are comparatively more necessary to them than to adults, and are carried on in a more than proportionate measure in their system. Old persons are drowsy, and find it difficult to keep themselves awake after food, yet court sleep in vain during the hours they have been accustomed in the earlier part of their life to expect it. This may be because their debilitated powers do not enable them to suspend the process of assimilation, and they are compelled to submit to its influence as soon as it operates; while the decay of nature, at the same time, evinces that the function in question is less constantly, regularly, or effectually performed: nor is the fact to be forgotten, that disturbed sleep and frightful dreams have frequently been ascribed to disorder of the digestive organs, whose preparatory office is indispensable to nutrition and assimilation.

It is true, that sleep after meals is most irresistible while the food is still in the stomach, after digestion has commenced, and long before assimilation has taken its turn. But we are ignorant how far the arrival of new matter in the blood-vessels may instantly contribute to the deposition of the old; as an additional number of balls put into a tube, at one extremity, will force out some of their predecessors at the other.

I enter into no argument on the subject. I repose on the rational presumption that sleep is something more than rest after fatigue—that it is probably the consequence of an important vital process in the delicate and fragile instruments of the mind—and that no process can be more requisite to those instruments, nor more likely to produce the effect, than the process of assimilation.

That this process is the proximate cause of sleep, receives the strongest confirmation from the facts detailed in the article SLEEP, in Rees's *Cyclopædia*, probably the last and best treatise on the subject, and which evidently points throughout to this cause, though the able writer of the article inadvertently suffered it to escape his attention. He appears to be satisfied with the common explanation, and adopts the unsatisfactory opinion that “the exhaustion of the powers of the animal organs, *by exercise*, is the determining cause of sleep*.” Yet he adverts to two facts, of a general nature and decisive importance, which subvert this opinion. “Some,” says he, “have called *fætal existence* a

* Rees's *Cyclopædia*, 23d vol. article SLEEP.

perpetual sleep; but the animal organs, *never having yet been exercised*, can hardly be said to be in a state of *repose* *." "The state of torpidity," he continues, "in which many animals pass the winter months, *cannot properly be called sleep*; it is not the *repose* of the animal organs *consequent on fatigue* produced by their exercise, but is a peculiar condition of the whole frame, affecting the internal as well as the external organs, and caused directly by the action of the cold †."

These two causes are alike in one particular, the sleep is not *repose after fatigue*. In the former, the process of assimilation is proceeding in every part of the frame, and in the brain as well as elsewhere—its operation, therefore, cannot be excluded from such participation in the phenomenon as it may be reasonable to assign it. In the latter case it is well known that the bear, the marmot, and other hybernating animals retire to their winter's repose in a state of corpulence and obesity, which they lose before they shake off their slumber in spring. They use little or no food in their retreat, yet the absorption of their superfluous flesh and fat may be applied by the assimilating process to the nourishment of the superior organs, including the brain. If the cold alone reduced them to a state of torpidity, by paralysing this organ, their emaciation would still remain to be accounted for; but the action of the absorbents and the process of assimilation remove every difficulty and explain every fact.

In complete sleep, hunger and thirst are not felt, as remarked by the same writer, "yet great hunger prevents sleep; and cold, affecting a *part* of the body, has the same effect. These causes operated on the unfortunate woman and her family, who lived thirty-four days in a small room overwhelmed by the snow, with the slightest sustenance: they hardly slept the whole time ‡." It does not clearly appear why the cold should have affected only a *part* of the body, and it is declared by this writer, "that intense cold, affecting *the whole body*, exhausts the animal powers and brings on sleep, which is speedily fatal §." The only facts certain in this account are, that they lived for thirty-four days with the slightest sustenance, and hardly slept the whole time. The process of assimilation might therefore have been only in proportion to the nourishment, and the want of sleep seems to be accounted for by the absence of this process. If the cold had affected only a part of the body, and was not intense, it might have kept the thoughts active, and the exercise of the brain might, for a time, have interrupted the process of assimilation, even if there had been the usual supply of nutriment; but if the cold had invaded the entire frame, and was in the highest state of inten-

* Recs's *Cyc. Ib.*

† *Id.*

‡ *Id.*

§ *Id.*

sity, its mode of action would probably have been, not to exhaust the animal powers, but to paralyse the brain itself. The sleep it would induce must therefore be speedily fatal.

But in the instance of this woman and her family, it may still be said, that the absence of sleep is sufficiently accounted for by the unintermitting pain of cold and hunger. But more violent pain than that of either could not postpone sleep for any considerable duration. "Even stripes and tortures cannot keep off sleep beyond a certain time*."

If a great exhaustion of the animal powers—or possibly, to advance a step nearer to the actual fact—if a great exhaustion of the *substance* of the brain and nerves should be the consequence of torture or over-exertion, such a state of those organs must be favourable to the occurrence of the assimilating process; and if there is a supply of nourishment in the frame, it naturally takes place, and the disturbance it necessarily creates in renewing those delicate substances, may be the occasion of sleep: if there is *no* nourishment to renew them, the consequence is not sleep, but death: if there *is* nourishment, and that the torture or the labour is beyond the strength of the individual to endure, the brain, as already mentioned in the case of excessive cold, becomes torpid and paralysed, and death, under these circumstances, also follows of course.

It is well worth inquiry, whether those various vegetable substances, which, being taken into the stomach, "bring on a condition of the brain favourable to sleep†," do not operate in the same manner; and in place of producing the process of assimilation, affect the brain with a temporary paralysis.—If they bring on this process, they must be useful auxiliaries—if *not*, and this is the more probable part of the dilemma, whatever be their apparent effects, they can only be prejudicial, unless where they are administered, not as soporifics but as anodynes.

The several circumstances just under review, afford an easy explication of the numerous facts detailed by the same writer. There is no reason to suppose that the process of assimilation had not materials to carry on its operations in "those boys who were completely exhausted by exertion, and fell asleep amid all the tumult of the battle of the Nile‡," nor in the soldiers "sleeping amid discharges of artillery, and all the tumult of war," "nor the couriers sleeping on horseback, nor coachmen on their coaches." This last is a very common phænomenon in this country; but I fear we must ascribe the peculiarity in question rather to the paralyzing effects of vegetable products taken into the stomach, than to the more wholesome accession of the assimilating process.

* Rees's *Cyc.*

† Id.

‡ Id.

Most of the other circumstances mentioned by this writer have already been adverted to, and they are all of easy and obvious explanation upon the proposed hypothesis; for example, “indigestion and various bodily affections produce sleeplessness*.” From preceding observations, it may readily be understood, that in digestion the nutritious matter continues in the stomach, instead of being carried into the system, and deposited in its due proportion in the brain and nerves. “All mental occupations attended with intense thought and great interest prevent sleep, and any great affections of the mind have the same effect†.” The solution of this phenomenon has already been given; but it is here to be noticed, that the very intensity of these meditations and passions in a certain time induces sleep. They exercise and exhaust the brain, and this exhaustion renders a renewal necessary by assimilation; and according to the hypothesis, this process cannot act on the substance of the brain, without occasioning sleep.

“A full repast is often followed by sleep, even in animals, as dogs. The distention of the stomach excites the circulation, and this brings on a condition of the brain favourable to sleep‡.” This condition, under the circumstances here noticed, can scarcely be any other than the activity of the assimilating process. “After the sleep has lasted long enough to restore the animal powers, we awake without any change or occurrence which can be shown to affect particularly the brain or other parts, of which the action was suspended by sleep§.” In other words, after the exhausted substance of the brain and nerves has been renewed by the assimilating process, we awake from the sleep which was the concomitant of its action. It would not be easy by any experiment to show that any change had taken place in those parts; but the fresh vigour with which we think and act is, in some degree, a proof of this change, and which is indeed implied in the very phraseology of this writer—“the restoration of the animal powers.”

He adds, “There are rare examples of individuals who have gone on sleeping for days, weeks, and months; but these histories are not accompanied with such particulars as would enable us to judge of the cause||.” It would be well worthy the attention of future inquirers to ascertain whether there are such facts in those cases, as would decide whether the cause of this state of torpor is not the protracted duration of the assimilating process: such, for instance, as the patient being overburthened with the obesity of a bear or a marmot in the commencement of his slumbers, and, like them, emaciated at their termination, without any other assignable cause for the change. It is, however, a

* Rees's *Cyc.* † Id. ‡ Id. § Id. || Id. well-

well-known fact, that corpulency predisposes to sleep, and sleep to corpulency. Does this happen because during sleep the process of assimilation is most active in every part of the frame? Having produced an exuberance of flesh and fat, these productions may become in their turn the cause of somnolency, by conducting to the more partial activity of the process in the brain; or at least the superfluous accumulation may form a kind of reservoir for the essential purpose of renovating the superior vital organs, when the usual measure of nutrition is no longer supplied.

Thus we may clearly comprehend the different, yet strangely analogous modes of action of vegetable poisons, intense cold, external injuries, and the assimilating process, on the brain. They all render it comatose, torpid, and paralysed; but none of them, except the last, are endowed with any but destructive powers. The assimilating process alone can renovate and restore the drained and exhausted organ: and even though the effect of its activity is to sink us in stupefaction, that very stupefaction is natural, refreshing, revivifying sleep.

A very formidable objection to this theory has, however, occurred to me. If the deposition of new matter by the blood-vessels creates such a disturbance in the brain, as to occasion the paralysis of sleep, why should not the action of the absorbents produce a similar effect, and, in removing the old matter, also bring on the same state of torpor and insensibility? That it does not, must be distinctly admitted; for the action of those vessels cannot but exist as well in the brain as elsewhere during our waking moments, and is probably most powerful during the intensity of thinking, as well as of bodily exercise. But if the hypothesis be true, this difficulty must admit of a solution. Can we then discover such a difference between the operation of these two actions on the brain, as will sufficiently account for circumstances so opposite?

In absorption, those particles which are removed, may leave the remaining cerebral mass in the very act of thinking, or at least not unfitted for the function. Every particle of the mass has already formed a part of the instrument destined to this office, and subservient to the exercise of one or other of the mental faculties. The new particles have never been exercised in any mode of thinking. They can differ but little, on their first arrival, from so many foreign bodies of equal dimensions; and is it surprising, that the oppression occasioned by their deposition should be felt throughout the delicate volume of the brain, until they are perfectly assimilated with the other particles, and fitted like them for mental operations?—a result which may, perhaps, in some measure be effected by the very sleep which they induce.

The

The nervous fluid and animal spirits have long since been excluded from all agency in the system. It is, therefore, scarcely necessary to advert to the antiquated theory of Haller, who, in seeking for the proximate cause of sleep, conjectures that this phænomenon “arises either from a simple absence, deficiency, and immobility of the spirits, or from compression of the nerves, and always from the motion of the spirits through the brain being impeded*.” “But that, if while the rest of the emporium of the senses and muscular motion is at rest, some part remains open, is pervaded by the spirits, and watches,” then, that our dreams occur; and also somnambulism, “if certain voluntary motions are conjoined with the perceptions of the mind†.” Yet, it is satisfactory to perceive, that all the explanation that this great physiologist endeavoured to derive from these imaginary essences, to satisfy his rational thirst of inquiry, may be found in the substantial reality of the brain and nerves—their partial exhaustion by exercise, and their indispensable renewal by the process of assimilation.

This diurnal operation may begin later, or cease earlier, in some portions of the brain and nerves than in others. Those portions, while exempt from its influence, may be as active as the entire system would be, were the individual awake. The thoughts which originate in these vigilant organs, not being compared by means of the senses with external objects, assume the substantial forms of reality, and constitute our dreams. Volition, as far as it is inherent in any organ, may exert itself to the extent of its power. But it can have no power to stimulate the neighbouring organs which continue asleep, or to put the limbs in motion, whose nerves remain subjected to the *assimilating process* which renders them torpid. But if those nerves have recovered from its effects, they will naturally submit to any volition accustomed to govern them—and this circumstance will account for all the perplexing mysteries of somnambulism. This phænomenon is of rare occurrence; and the nerves of motion are so seldom exerted in sleep, that hitherto the will has *then* been supposed in a state of abeyance. But we can much more rationally account for the various phænomena of dreams, night-mare and somnambulism, by supposing that the will may be active in any of the cerebral organs which happen to be awake, yet destitute of power to put the limbs in motion as long as the nerves of those limbs are involved in the stupor of sleep, and invested with this power from the moment that the stupor in question is removed from those nerves. But it is not to be forgotten, that if the brain be altogether paralysed by sleep, so must the whole body. Som-

* Haller's First Lines of Physiology, p. 285. Edinburgh, 1801.

† Id. p. 283.

nambulism, therefore, can only take place when part of the brain is awake, and in communication with the nerves of locomotion. These considerations, with others detailed above, will also suffice to account for the fact recorded by the excellent writer so often referred to, "that many soldiers, in the retreat of Sir John Moore, fell asleep on the march, and still continued to walk with their comrades*."

But whatever may be thought of these speculations, there is no difficulty in comprehending Dr. Spurzheim's development of the nature of dreaming: and if we are acquainted with the inadequate theories of his predecessors, to comprehend his explanation is but an easy step to its unqualified adoption. It accounts for every phenomenon connected with the subject, hitherto unexplained. If the whole brain is locked up in sleep, there is no dream. If a portion of it is emancipated, thoughts peculiar to that portion arise, and those thoughts are dreams. The mechanic's imagination may rove among machinery, the mathematician may solve a problem, the orator pour forth unstudied eloquence, the poet unpremeditated verse, the wit delectable jests, the musician unprecedented harmony; yet this does not always occur, but occasionally. If the peculiar organ happens to be asleep, there is no music, no wit, no poetry, no oratory, no mathematics, no mechanics—a different faculty may be active, and these individuals may wander through inextricable difficulties, or fly before wild beasts, or combat with enraged assailants, or dissolve in a cold sweat at the frightful visit of some spectre from the grave. It is not because the organ may have been frequently or recently exercised, that it is employed in a dream; it is simply because it has escaped from the trammels of sleep which still envelop the remainder of the brain, or at least the senses, which open a communication with the external world, and supply the only means by which we are informed whether similar objects of thought are realities or illusions. This theory, therefore, explains why we sometimes have dreams and are sometimes without them—why we sometimes dream on the subject most familiar to our reflections, and sometimes ramble into the most unaccountable fancies—and lastly, why happiness and misery are occasionally the companions of our sleep, according as peculiar organs are gently affected or rudely agitated by the thoughts which engage them—pleasure frequently losing itself in pain, as the mental disturbance increases, till at length the accumulating uneasiness trespasses on the sensorium, or the very organs of sense, when, suddenly awaking, we find an unexpected relief from our griefs, vexations, and terrors.

[To be continued.]

* Rees's *Cyc.* article SLEEP.

XLIII. *Principles of finding the Longitude.* By A CORRESPONDENT.

To Mr. Tillock.

SIR, — AFTER all the labour which has been bestowed to render the present practice of ascertaining the longitude worthy of general estimation, to institute another method by which the complete solution of that problem can be attained, may seem unwarrantably bold; but, while the author's conviction of its truth has urged him to seek for an opportunity of making it public, a consciousness of his attainments being very limited in comparison with the great acquirements of those who have already pursued this interesting investigation, compels him to acknowledge that to a fortuitous thought rather than to an extensive acquaintance with science, are to be ascribed the researches which he thus ventures to present.

That his suggestions may be candidly criticized, and any theoretical errors or impracticable proposals pointed out by those more conversant with astronomical pursuits, he requests with respectful deference.

Definition of the Subject.

To discover the longitude of any place is to find out what meridian passes through it, and to calculate how far *that* and the *first meridian* are apart. And this should be done by a process as simple as the case allows, one that is often at command, and which can be performed with a requisite dispatch to the certainty of the operation.

From the nature of the question, the means by which it is determinable must have relation to the first meridian.

Now, the relation which subsists between the fixed stars and the first meridian is direct and constant; for once in every revolution of the earth, the same stars are vertical to the same places on its surface; and not only does the earth fulfil every revolution on its axis in the same time, but also revolves with an equable motion throughout: whereas its motion in the annual revolution varies as the progress from equinox to equinox.

Hence, every star will describe equal arcs of its circle of apparent revolution in equal times; and any arc of a star's circle of revolution will be to the time in which the star describes that arc, proportionately as the whole circle, or 360 degrees, is to the time in which the star completes a revolution.

Again: the terraqueous globe and the apparent orb of the fixed stars are concentric; and they may be said to revolve in regard of each other conversely; so that if a meridian of each sphere coincide

coincide in one plane, those meridians will in the same time remove—the earth's to the east, the stars to the west of the meridian previously opposite—in such sort as that the *then* and *now* opposed meridians of each sphere will be an equal number of their respective degrees apart.

The duration of the earth's revolution around its axis is, in mean *solar* time, Hours 23 56' 4" 7''' 13^{ivths} 13^{vths} 36^{viths}; which lapse of TIME may be measured by an index *once* tracing the periphery of a dial circle, graduated, and also divided into parts of 10° figured 1 to 36, to assist the eye whilst inspecting the quantity of an arc.

By this MEASURER of *time* in accord with the relative motions of the fixed stars and the earth, let the instants of the culminations of conspicuous stars be successively noted in the manner of a table;—with this MEASURER and the *table*, an observer, wherever situate on the globe, can quickly ascertain how much westward or eastward of the first meridian *our meridian* is; and whether it bears east or west from the first. For the index, the stars, the globe really, all move in equable and known revolution, *i. e.* the index, any star but those which are precisely vertical at the poles,—any place on the globe's surface but the poles,—all pass through 360 degrees in the same time, and consequently through equal arcs of their respective circles of revolution in the same portion of that time; wherefore, regarding what time one of those conspicuous stars culminates to the place for which the longitude is required, we have at once the information sought; the arc contained between the part of the dial circle to which the index of the MEASURER at that instant points, and that to which it points when the star culminates at the first meridian (known by the *table*), *being equal to the longitudinal arc on the globe between the first meridian and the meridian of the place*; and the place is *west* or *EAST*, according as the present instant is in the succession of the parts of TIME *nearer* to the moment when the star *last* culminated at the first meridian, or to the moment when it will culminate there NEXT.

No objection on account of the variability of chronometers from change of climate, or other causes, can hold good against the means here proposed, that will not at the same time impugn the methods now in use. It is to be recollected also, that the high degree of accuracy to which time-keepers can now be brought, is probably but a trifle less than the utmost precision which the constitution of the materials best adapted for this purpose will allow the art of man to gain.

Further, the advantage of the method now advanced, in leaving to the mariner a short and plain practice, is very considerable; for

for the more numerous are the objects to which the attention must be directed, the more likely is the chance of error arising and increasing from the intricacy of combination. In short, the principles here set forth, if unacknowledged in the usual directions, must nevertheless be latent in any mode of approximating to the true answer of this great problem in navigation; it only then remains to be desired, that the most compendious way of so far solving it as is possible, may be adopted.

In what was said, the meridian of the place was supposed to have been already determined by one of the commonly practised methods: as, however, there are times when no other heavenly bodies but the stars are visible, it may not be deemed inapplicable, to show how that spangled canopy alone affords sufficient clue for finding the mid-heaven arch above any diameter of the compass.

The subjoined rules depend upon the zenith point being correctly distinguished; which is always the postulate of marine observations.

1st. It being known that two (or more) stars have the same longitude, *i. e.* lie south and north of one another, when they are in a line with the zenith the meridian coincides therewith; and when they are not in a line with the zenith, yet a line drawn through the zenith parallel to the line in which they are is identical with the meridian. When sufficiently elevated and clear to view, either pole and the zenith give the meridian line.

2d. It being known that two (or more) stars have the same latitude, *i. e.* lie west and east of one another, when they are equally above the horizon, the line that joins them is to be equally bisected by a perpendicular to the same,—the bisecting line is in the plane of the true meridian.

1819.

W. W.

XLIV. *Researches on some important Points of the Theory of Heat.* By MM. PETIT and DULONG*.

CONVINCED that certain properties of matter would exhibit themselves under simpler forms, and might be expressed by more regular and less complex laws, could we but refer them to the elements on which they immediately depend, we have endeavoured to introduce into the study of some of the properties which appear more intimately connected with the individual action of the material molecules, the most certain results of the atomic theory. We are led to hope, from the success we have already met with, that the atomic theory will receive from our investigation a new

* From *Annales de Chimie et Phys.* tome x.

degree of probability, and sure methods of determining the truth among different, equally probable, hypotheses.

By directing our observations in a suitable manner we have discovered simple relations between phænomena, the connexion of which had not been previously attended to. The many points of view under which these phænomena may be considered, preclude our embracing the whole at one time; but we have thought that it might be useful, in the interim, to make known the results we have obtained.

These first results relate to specific heats. The determination of this has been the object of the labours of many philosophers. The attempts hitherto made to discover some laws in the specific heats of bodies have all been unsuccessful. It is not uncommon, for example, to meet with numbers, in the best tables, three or four times as great as they ought to be.

Our first care was directed to what could render the measurements that we were to use as accurate as possible. Among the methods of determining the capacities of bodies, those in which the melting of ice, or the mixing of bodies with water, are employed, may doubtless, if properly conducted, lead to very exact results; but most of the substances on which it is indispensable to operate, can seldom be obtained in sufficient volume to enable us to apply these methods; and it was therefore necessary that we should have recourse to a different one. That which we have chosen appears to unite all the requisite conditions. It is founded upon the law of cooling.

It is well known that there exist between the times of cooling of different bodies placed in the same circumstances, and the specific heats of the same bodies, relations in consequence of which the ratio of the capacities may be deduced from that of the times of cooling. Mayer first applied this principle, and satisfied himself that the capacities determined in this way differ little from those obtained for the same bodies by the method of mixture. Leslie, who adopted this method, pointed out an additional precaution, of which Mayer did not suspect the necessity; viz. to inclose the body operated on in an envelope, which must always be the same, to prevent the error which would result from inequality in the radiating power of the surfaces. The most important, however, of all the causes of uncertainty, and to which neither of these philosophers paid any attention, is that, which results from the unequal conductibility of the substances that are compared. This cause has the less influence the smaller the volume of the bodies on which we operate, and the slower the heat is permitted to make its escape. The aim then should be to fulfil both of these conditions; but it is difficult to reconcile them, because, when we diminish the volume of the body, we augment the velocity

city with which the heat is dissipated. However, by uniting the different causes which contribute to retard the cooling of a given mass, we are enabled, as our experiments have proved, to place it in such circumstances that the differences in the conductivity of the substances operated upon, have no sensible influence on the measure of the capacities.

To attain this end, the most obvious method is not to begin the observation till the temperature of the body is only a few degrees higher than that of the surrounding bodies. All our experiments, therefore, were made in temperatures between 10° and 15° (centigrade) above the surrounding medium. The changes of temperature should be measured with the greatest care; for even a slight error in the estimate might occasion a great one in the result. By operating, as we have stated, at the same temperature for all the bodies, we avoid errors resulting from the graduation of the thermometer; and by observing this instrument through a lens, we can increase the size of the degrees so much as not to commit an error exceeding the 50th of a degree,—a quantity so minute that it may be disregarded. To obtain uniformity of temperature in the ambient medium during the whole time of every experiment, the body was always placed in a vessel whose sides were blackened interiorly, and covered on all parts with a thick coating of melting ice.

To this first means for diminishing the rate of cooling we added another, the influence of which we could calculate from our knowledge of the laws of the communication of heat. From these laws it results that the velocity of cooling of a body may, *cæteris paribus*, be considerably diminished when its surface possesses but a very weak radiating power, and is immersed in an air very much dilated. To accomplish this, we determined to operate on solid bodies only in a state of very fine powder. In this state they were strongly pressed into a cylindrical vessel of silver, very thin, very small, and the axis of which was occupied by the bulb of the thermometer. This silver cylinder was then placed in the centre of the vessel, the air contained in which was rarefied till its tension did not exceed two millimetres; and care was taken to reproduce the same rarefaction in each experiment.

We thus succeeded in making the cooling of very small bodies go on very slowly, and consequently easy to be observed with precision. It is sufficient to say, that when measuring the capacities of the densest bodies, as gold and platinum, the quantities on which we operated did not exceed 30 grammes; and that the time of cooling was never less than 15 minutes.

We ought now to give the formula which served for the calculation, but the details would lead us into a discussion which we reserve for the publication of the definitive results of all the
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direct experiments which we have made on the subject. We add only a single remark ; that having compared the specific heats thus obtained for the worst conductors with those given by the method of mixture, or by the calorimeter, their agreement has afforded the most convincing proof of the accuracy of our process. We shall now present in a table the specific heat of several simple bodies, restricting ourselves to those results concerning which we entertain no doubt.

	Specific Heats, that of Water being 1.	Weight of the Atoms, that of Oxygen being 1.	Product of the Weight of each Atom by the corresponding Ca- pacity.
Bismuth ..	0.0288	13.300	0.3830
Lead	0.0293	12.950	0.3794
Gold	0.0298	12.430	0.3704
Platinum .	0.0314	11.160	0.3740
Tin	0.0514	7.350	0.3779
Silver	0.0557	6.750	0.3759
Zinc	0.0927	4.030	0.3736
Tellurium .	0.0912	4.030	0.3675
Copper ..	0.0949	3.957	0.3755
Nickel ...	0.1035	3.690	0.3819
Iron	0.1100	3.392	0.3731
Cobalt ...	0.1498	2.460	0.3685
Sulphur ..	0.1880	2.011	0.3780

To render the law which we propose to make known intelligible, we have, in the preceding table, joined to the specific heats of the different bodies the relative weights of their atoms. These, as is known, are deduced from the ratios observed between the weights of the elementary substances that unite together. The pains taken for some years past to determine the proportions of most chemical compounds, leave but slight uncertainties respecting the data which we have employed; but as no precise method exists of discovering the real number of atoms of each kind which enter into a combination, there must always be something arbitrary in the choice of the specific weight of the elementary molecules : this uncertainty, however, can only be in the choice of two or three numbers which have the most simple relation to each other. The reasons which have directed our choice will be understood from what follows. There is none of the numbers on which we have fixed which does not agree with the best established chemical analogies.

From the data contained in the preceding table we may now easily calculate the ratio which exists between the capacity of atoms of a different kind. In order to pass from the specific heats

heats furnished by the observations of those of the particles themselves, it is sufficient to divide the former by the number of particles contained in the same weight of the substances which we compare: but it is obvious that the number of particles for equal weights of matter are reciprocally proportional to the density of the atoms. We shall, therefore, obtain the desired result by multiplying each of the capacities deduced from experiment by the weight of the corresponding atom. These different products are presented in the last column of the table.

The approximation apparent on a bare inspection is too remarkable by its simplicity, not to indicate the existence of a physical law capable of being generalized and extended to all elementary substances. These products, which express the capacities of the different atoms, approach so near equality, that the slight differences must be owing to trifling errors either in the measurement of the capacities or in the chemical analyses; especially if we consider that, in certain cases, these errors, derived from these two sources, may be on the same side, and, consequently, be found multiplied in the result. The number and variety of the substances on which we operated not allowing us to consider the relation thus indicated as merely accidental, we are authorized to deduce from them the following law:—*The atoms of all simple bodies have precisely the same capacity for heat.*

By recollecting what has been stated respecting the kind of uncertainty that exists in fixing the specific weight of the atoms, it may be easily conceived that the law which we have just established will change, if we adopt for the density of the particles a supposition different from what we have chosen; but, in every case, the law will exhibit a simple ratio between the weights and the specific heats of the elementary atoms; and it is obvious that, when we had to choose among hypotheses equally probable, we should naturally be led to prefer that which established the most simple relation between the elements which we compared. But whatever opinion be adopted respecting this relation, it will enable us afterwards to control the results of chemical analysis; and, in certain cases, will give us the most exact method of arriving at the knowledge of the proportions of certain combinations: but if, in our subsequent experiments, no fact occur to invalidate the probability of the opinion we now hold, we shall find, in this method, the advantage of fixing in a certain and uniform manner the specific weight of the atoms of all simple bodies that can be submitted to direct observations.

The law we have announced, seems to be independent of the form which bodies assume, provided that we always consider them under the same circumstances. This, at least, is a consequence deducible from the experiments of MM. Laroche and Berard on
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the specific heat of the gases. The numbers given by them for oxygen and azotic gases do not differ from what they ought to be to agree accurately with our law, except by a quantity less than the probable errors of such experiments. The number for hydrogen is rather too small; but on examining, with attention, all the corrections which the authors were obliged to make on the immediate results of their observations, it may easily be seen that the quickness with which hydrogen lowers to the temperature of the surrounding bodies, compared with other elastic fluids, ought necessarily to introduce into the determination relative to that gas an inaccuracy from which they did not attempt to free it. By taking into consideration this source of error, we are enabled to explain the difference to which we have alluded, without being compelled to make any false supposition.

Having thus established the law of specific heats for elementary bodies, it became very important to examine, under the same point of view, the specific heats of compound bodies. Our process applying indifferently to all substances, whatever their conductivity or state of aggregation may be, we were enabled to subject to experiment many bodies whose proportions may be considered as fixed; but when we attempt to ascend from these determinations to that of the specific heat of each compound atom, by a method analogous to that which we employed for the simple bodies, we find ourselves soon stopped by the number of equally probable suppositions among which we must make our election. Since the method of fixing the weight of the atoms of simple bodies has not yet been subjected to any fixed rule, that of the atoms of compound bodies has been, *à fortiori*, deduced from suppositions purely arbitrary. But instead of adding our own to the conjectures before advanced on the subject, we choose rather to wait till the new order of considerations which we have established can be applied to a sufficiently great number of bodies, and in circumstances sufficiently varied to place the opinions that may be adopted, on decisive conclusions. For the present we shall only remark, that in abstracting each particular supposition, the observations we have hitherto made tend to establish this remarkable law,—*that there always exists a very simple ratio between the capacity of the compound atoms and that of the elementary atoms.*

Another consequence very important for the general theory of chemical actions may likewise be deduced from our researches; namely, that *the quantity of heat developed at the instant of the combination of bodies has no relation to the capacity of the elements: and that in the greater number of cases, this loss of heat is not followed by any diminution in the capacity of the compounds formed.* Thus, for example, the combination of oxygen

gen and hydrogen, or of sulphur and lead, which produces so great a quantity of heat, occasions no greater alteration in the capacity of water, or of sulphuret of lead, than the combination of oxygen with copper, lead, silver,—or of sulphur with carbon, produces in the capacity of the oxides of these metals, or of carburet of sulphur.

These facts cannot be easily reconciled with the generally received ideas respecting the production of heat in chemical phenomena; for, to do so, it would be necessary to admit the improbable supposition that heat exists in bodies in two very different states, and that the portion which we consider as united to the particles of matter is entirely independent of the specific heats. There is, besides, much vagueness and incoherence in the explanations relative to the kind of phenomena of which we speak. The opinions entertained respecting them differ so widely that they can neither be regularly discussed, nor exposed to complete refutation.—It may perhaps be useful to recall briefly the principal facts, and the inductions belonging to this important branch of the science.

Of all the chemical actions considered as sources of heat, none were recognised till lately, except combustion. To search for a plausible theory for this mode of producing heat, before the epoch marked by the memorable discoveries of Lavoisier, would be folly. This illustrious chemist, having more particularly considered the action of oxygen in the state of gas, formed an opinion respecting the cause of this phenomenon, suggested by the observations of Black on latent heat. Hence the idea that the heat liberated during combustion comes from the change of state of the oxygen. The determination which, in concert with Laplace, he made, of the quantities of heat disengaged by the combustion of several substances, appeared to furnish a powerful argument in favour of his conjectures; for experiment showed that when the same quantity of oxygen was united, successively, with phosphorus, hydrogen, and carbon, it disengaged more heat in the first case than in the second, and more in the second than in the third. This might have been expected from the theory; the result of the first combustion being solid, that of the second liquid, and that of the third gaseous: but on considering that the two elements which concur to produce water, lose both the gaseous state, and that, notwithstanding, the heat developed is less than what results from the combustion of phosphorus naturally solid, he was necessarily led to conclude that the latent heat of oxygen must be superior to that of the other elastic fluids. Another difficulty soon after presented itself: nitric acid, in which the oxygen has already lost the gaseous form, and still more nitre, which is in a solid state, produce, when decomposed by combus-

tibles, quantities of heat very different from what would be produced by a weight of gaseous oxygen equal to that which they contain. This fact, which ought to have excited doubts respecting the first explanation, only restricted its generality: it was then supposed that the oxygen, in certain combinations, was capable of retaining a dose of caloric almost as great as that which it contains in the elastic state. Later observed facts could not be explained according to the theory, without admitting that oxygen in certain combinations retained a quantity of heat even superior to what it contains when in the elastic state: such are the detonations produced by mixtures of chlorate of potash with certain combustibles, or the spontaneous explosions of Davy's euchlorine, and of the chloroide and iodide of azote.

This mode of explanation was afterwards extended to all combinations. It was considered as a principle sufficiently established, that a body, in combining with a certain number of others, might abandon a greater or less portion of its heat, according as, in each case, the different degrees of affinity of the elements in contact occasioned the molecules to approach more or less nearly to each other. It is the degree of this approach, essentially variable, that has been designated by the word *condensation*, so frequently employed by chemists. This is the theory adopted almost generally in France. Several foreign chemists have pointed out its inaccuracy, and modified it in several points, but without producing any conclusive proof, either against the opinion which they combat, or in support of that which they would substitute.

It thus appears that the different explanations relative to the development of heat, in chemical combinations, are reducible to simple assertions derived from the first hypothesis of Lavoisier: and it is wonderful that, since this doctrine was first proposed, it has not been more closely examined; and that, even from the results already known, all the arguments which they are capable of furnishing against it have not been drawn from them. The relations which we have pointed out between the specific heats of simple bodies and those of their compounds, preclude, we think, the possibility of supposing that the heat developed by chemical actions owes its origin merely to the heat produced by changes of state, or to that supposed to be combined with the material molecules. We have even a better reason for rejecting this purely gratuitous hypothesis, as we can explain the phenomenon in a manner more satisfactory. In fact, Davy has long ago shown that when the two poles of a Voltaic pile are united by means of pieces of charcoal placed in a gas incapable of supporting combustion, the charcoal may be kept in a state of strong ignition as long as the pile remains in activity, and without the charcoal undergoing any chemical change. On the other hand, we are warranted to conclude,

conclude, from many Galvanic experiments made by Hissinger and Berzelius, and by Davy, that all bodies which combine are, with respect to each other, at the moment of combination, precisely in the same electric conditions as the two poles of the pile. Is it not then probable that the cause which produces the incandescence of the charcoal in the beautiful experiment just mentioned, is likewise the cause of the greater or less elevation of temperature of a body during the act of combustion? At least this conclusion is founded on the strongest analogies, and ought to be followed through all its consequences. We by no means contend that the changes of constitution which result from chemical combinations have no part in the development of heat with which they are accompanied: we only mean to say that, in very energetic combinations, this cause produces, in general, but a very small part of the total effect.

In closing this memoir, we cannot pass in silence another very important application, to which the exact knowledge of the specific weight of the atoms will lead. If, as we have reason to think, we have by the foregoing considerations succeeded in determining this element with accuracy, we may, setting out from the proper densities of bodies, calculate the ratios which exist between the distances of their atoms: and it is easy to see how important it will be, in many physical theories, to be able to establish a comparison between the distances of the particles, and certain phenomena which may naturally be supposed to stand connected with the new element. For example, it is by examining the question of the dilatations under this new point of view, that we may expect to arrive at simple laws, at present quite unknown. Some essays made on the observations of different philosophers, and on some of our own, (made with a different object,) lead us to consider it very probable that there exists a simple relation between the dilatability of liquids and the distances of their particles. The fine observations of Gay-Lussac on the identity of the contractions of carburet of sulphur and alcohol, setting out from their respective boiling points, support our opinion; for these two liquids present this remarkable particular, that, at the temperatures at which they were compared, the distances between their particles are nearly identical. Before, however, pursuing the researches on this subject, it will be necessary to elucidate, as much as possible, the question of specific heats, and to deduce from it all the consequences to which it may lead relative to the knowledge of the constitution of bodies.

XLV. *On Irregularities observed in the Direction of the Compass Needles of H. M. SS. ISABELLA and ALEXANDER, in their late Voyage of Discovery, and caused by the Attraction of the Iron contained in the Ships. By Captain EDWARD SABINE, of the Royal Regiment of Artillery, F.R.S. &c.**

IT is proposed in this paper to show in what respects the effects of local attraction, in the abovementioned ships, were conformable to the observations which had been made in preceding voyages; and how far the errors, which were found to take place on different courses, and under different dips of the magnetic needle, corresponded with the rules for calculating corrections, which Captain Flinders had found useful in his own experience, and which he had recommended for a more extensive trial.

It may be desirable to premise, that the irregularities here alluded to, are not those accidental disturbances which may be caused by iron placed inadvertently too near the compasses; but the permanent and constant effect of the mass of iron contained in a ship, affecting its compasses at all times, and in a greater or less degree, according as its influence is more or less powerful, in comparison to the directive force of the earth's magnetism. That errors have always existed from this cause, may be inferred, from the uncertainty which experience has attached to the results of azimuths observed in ships. The cause, however, appears to have been very long unsuspected, whilst its effects had produced a general impresson, that the azimuth compass was in itself an imperfect instrument, and only to be relied on within certain undefined and variable limits.

It was reserved to the accurate observation, and the habit of recording and comparing apparently trivial and accidental differences in results, which distinguished the late Mr. Wales, (astronomer in the second voyage of Captain Cook,) to enable him to lead the way to a knowledge of the nature and causes of these errors: he remarks, "that in the passage of the Resolution and Adventure to the Cape of Good Hope, and subsequently, the greatest west variations had happened when the ship's head was north and easterly, and the least when it was south and westerly, differing very materially from one another with the ship's head in different positions, and still more when observed in different ships;" thus manifesting that they were something more than accidental.

This voyage was the last in which Mr. Wales embarked, and the investigation does not appear to have been pursued in this country until the voyage of discovery to Terra Australis, in the

* From the Philosophical Transactions for 1819, Part I.

first years of the present century. The survey of the coast of New Holland being carried on in a considerable measure by the intersection of compass bearings taken from the deck of the Investigator, so much embarrassment and perplexity were found to arise from the effects of local attraction, that much of Captain Flinders's attention and thoughts were necessarily devoted to a consideration of some means of remedying the inconvenience.

On his return to England, he obtained permission from the Lords Commissioners of the Admiralty to make a course of experiments in ships under their direction at the principal sea-ports, with a view to ascertain if compasses were similarly affected in other ships, and to try the general applicability of rules which he had found useful in correcting the errors in the Investigator. These rules, with the observations and reasonings on which they were founded, were published in a short paper in the Philosophical Transactions, and in a more detailed form in Appendix No. II. in the Voyage to Terra Australis. There are three points in these statements chiefly worthy of attention, from their practical importance; and on which it seems desirable, therefore, to notice how far his observations have been confirmed by those made in the Isabella and Alexander.

First; he found that in every ship a compass would differ very materially from itself, on being removed from one part of the ship to another. Experience of this source of irregularity had induced him, early in his voyage, to confine the use of the compass, with which his survey was carried on, to one particular spot. The place he selected was determined by convenience in other respects; it was on the binnacle, and exactly amidships.

The Isabella and Alexander had not completed half their voyage across the Atlantic, before it was found that the binnacle compasses of the one ship differed very materially, in indicating the course steered, from those of the other: namely, one point, or $11\frac{1}{4}^{\circ}$. No dependance whatsoever could be placed on the agreement of compasses in different parts of the ship, or of the same compass with *itself*, if removed but a few inches: even in the neighbourhood of the binnacles the variation, as observed amidships, was from 8° to 10° greater than the result of azimuths taken by a compass placed between two or three feet on the larboard side; and an almost equal difference in a contrary direction took place on removing the compass to the starboard side, rendering it a matter of some trouble and difficulty, to make the azimuth compass agree with those in the binnacle by which the ship was steered, and for which it was therefore necessary to determine the variation.

As the ships ascended Davis's Strait, these latter compasses began to traverse so sluggishly, that it was necessary to shake the

binnacles continually to assist their motion. The cards of these* had a metal rim round their circumference, weighing one ounce eleven drachins avoirdupois, which, as the directive power of magnetism diminished, became too heavy for the needle to carry round: they were also frequently found to disagree with each other from $\frac{1}{4}$ to $\frac{3}{4}$ of a point; the consequence, most probably, of the different local attraction to which they were exposed. These compasses ceased therefore to be attended to, except as an occasional assistance to the helmsman, and a position was selected in each ship, in which a compass on a more suitable construction was permanently fixed; by this the ship's course was directed, azimuths taken, and bearings of land, &c. noted, during the voyage.

This *standard compass*, as it may be called, was placed in the *Isabella* exactly amidships, between the main and mizen mast, on a stout cross beam elevated nine or ten feet above the deck; this beam was the usual walk of the Greenland pilot, or of the quarter master, as affording a better view of the ice among which the ships were frequently steered, than from the deck. The elevation was an advantage to the compass in such high magnetic latitudes, by rendering it less liable to accidental disturbance, on the removal of such implements of iron as were required to be kept on deck for use. The *Alexander* not having a similar cross beam, her compass was fixed amidships on a box of sand placed on the companion, between five and six feet above the deck.

Secondly; Captain Flinders found that in his compass permanently fixed as described, no error took place when the ship's head was on the magnetic north or south points; showing, that at such times the attraction of the ship, and of magnetism, were in the same line of direction. The maximum of error also took place when the ship's head was at right-angles to these points; namely, at east or west; being however in opposite directions, in excess of the true variation on the one side. and in defect on the other; so that the extreme difference occasioned by altering the course from east to west, or the reverse, would be twice the error at either.

On the intermediate points, the ratio of the error to its maximum, was as the "sine of the angle between the ship's head and the magnetic meridian to the sine of eight points, or radius," or sufficiently near to admit of corrections being calculated for every course, when the error on a single one was known by observation.

Thus far the experiments which Captain Flinders tried in every ship corresponded, excepting only that the maximum of error in different ships at the same place would differ materially.

* Burt's patent binnacle compasses.

The accordance in so many ships gave him reason to believe that in compasses placed near the binnacle, and amidships, *the points of no error* would be most commonly those of the magnetic meridian. Considering, however, that this must depend altogether on the distribution of iron, and may be therefore liable to great diversity, he recommends that in every ship, as soon as a fixed position has been selected for a compass, the points of no error should be determined by repeated observation. The method that was adopted for this purpose in the late voyage appearing both simple and effectual, it may be useful to exemplify it by an instance or two.

The *Isabella* being at anchor in Brassa Sound, Shetland, her head was placed, by means of warps, on each point of the compass successively, and the bearing of a pile of stones on the summit of a distant hill noted by her compass at each point : at the same time that these observations were made on board, her bearing from the hill was also observed by a compass placed on the pile of stones : the *agreement* in bearing showed the points of no error, and the *differences* the errors in each point, without the calculation which azimuths involve.—[See Pl. IV. fig. 1.]

The *Alexander* being along side a floe of ice in Baffin's Bay, the true magnetic bearing from the ship, of a very distant and well defined object on the main land, was found by carrying a compass on the ice in an opposite direction, to a distance which ensured its being perfectly free from local influence. The ship's head being then warped round to each point of the compass successively, the errors in each were determined by the difference in bearing, as in the last instance.—[See Pl. IV. fig. 2.]

The regularity in the above results is the best testimony that the method is a satisfactory one. Certain precautions must be attended to : thus, the object must be sufficiently distant, that the space occupied in warping the ship round may not subtend any sensible parallax. The direction of the ship's head should be noted by the compass by which the bearings are taken. A short time must be allowed to elapse after the ship is steady on any point, to ensure the traversing of the cards : this is particularly necessary in high latitudes when the compasses move very sluggishly. And lastly, the observations should be repeated.

It will be observed by the above results, in the *Isabella* and *Alexander*, that the points of no error were not coincident in either ship with those of the magnetic meridian ; in the *Alexander* especially, they were more nearly at right angles to it. That this ship should have differed so materially from all the instances on record, may be attributed to her compass being so near the level of the deck, and therefore being more affected by the influence of a considerable quantity of iron articles (such as ice anchors,

ice saws, &c.) which were carried on the after part of the deck for convenience in use, than it would have been, had it been raised higher. This was proved by placing a compass on a plank elevated for experiment in front of the companion, to the same height as in the *Isabella*, namely, nine or ten feet. The points of no error were found, in this position, to be about north and south, and the amount of error at eight points, nearly twenty degrees; the same as in the *Isabella*: the greatest error at the same time, by the *Alexander's* standard compass, viz. the one nearer the decks, being $8^{\circ} 20'$ at N.N.E. The dip was $84^{\circ} 09'$.

The propriety of Captain Flinders's recommendation, to determine the points of no error in a fixed compass by actual observation in every ship as soon as the distribution of iron is completed, may therefore be considered as confirmed by the observations in the *Isabella* and *Alexander*; whilst his rule of proportion may receive a verbal alteration to render it more suitable for general application: so corrected, it would be as follows—the expressions substituted being marked in italics, and the original words entered in the margin.

East or west.	“The error produced in any direction of the ship's head, will be to the error at <i>the point of the greatest irregularity</i> , as the sine of the
Magnetic meridian.	angle between the ship's head and <i>the points of no error</i> to the sine of eight points, or radius.”

Thirdly; Capt. Flinders's experience in the *Investigator* showed that the maximum of error in the same compass would be different in different parts of the world, although the use of the compass was confined to one particular spot in the ship, and every precaution taken to avoid an interference with the distribution of the ship's iron.

It is worthy of remark, that by multiplying observations and by comparing the series one with another, he was thus *practically* led to trace a connection between the amount of the errors and the dip of the needle; a knowledge of the fact preceding, in his mind, any theoretical suggestion that such might be the case.

It does not appear indeed that the principal cause of this connection was even subsequently known to him; he perceived that the influence of local attraction on the compass needle increased as the dip became greater. He endeavoured to account for this circumstance, on a supposition that all iron might receive an *absolute* increase in the intensity of its attractive power by approaching the magnetic pole.

The increase, however, which was the subject of his observation, was a *relative* one, being in comparison to the directive power of magnetism. A diminution in the latter would therefore produce the effect equally with an absolute augmentation in the former;

mer; and that such a diminution does take place, and in a degree which is sufficient to account for all the effects observed, will be evident to every person, who reflects that although the magnetic force is greatest at the pole, its directive power must then have wholly ceased, having become less on the horizontal traversing of the needle in proportion as the point of attraction has been brought beneath the compass; indicated by the angle which the dipping needle makes with the horizon. This is doubtless the principal cause of the connection which Captain Flinders was the first to trace.

It is not designed to say that this cause may not be aided by the increased magnetism of portions of the ship's iron, such as bars and stanchions; which being fixed in an upright position, may receive an addition to their attractive power where the position of the dipping needle is always coincident with theirs; but merely to observe, that a cause is known to exist for the connection, independently of supposition; which cause, conjointly with experience, shows the inadequacy of the rule proposed by Captain Flinders, whereby the amount of error, under any known dip, being ascertained, the amount may be calculated for any other dip, by using as a multiplier, the decimal expression of the proportion which the error, in the one ascertained instance, may have borne to the dip.

In the observations made in the *Isabella* at Shetland, where the dip is $74^{\circ} 21\frac{1}{2}'$, the maximum of error was $5^{\circ} 34'$ easterly of the true variation, with the ship's head at E.S.E. and $5^{\circ} 46'$ westerly at W.N.W. making an extreme difference of $11^{\circ} 20'$.

By Captain Flinders's rule, the common multiplier for this compass would have been about one twelfth, or $\cdot 083$, which at a dip of $86^{\circ} 09'$, which was the greatest observed during the late voyage, would have given an error of between 7° and 8° , making the extreme difference 15° ; whereas repeated observation showed it to be at that time more nearly $50'$, if not exceeding that amount.

The inadequacy of the rule will also appear by reference to the observations made by the *Alexander* in Baffin's Bay. The error at eight points being $6^{\circ} 46'$, at a dip of $84^{\circ} 30'$; it ought scarcely to have exceeded 7° at the greatest possible dip, making an extreme difference of less than 15° . No opportunity occurred indeed of making accurate observations at a greater dip than the above; but the difference in the bearing of objects before and after tacking indicated with sufficient certainty, that the error had increased to an amount very far beyond 15° ; frequent instances of an extreme difference of from 3 to 4 points being remarked, as the ship approached the furthest western longitude to which she attained in a high latitude; this was in Lancaster's Sound

Sound of Baffin, into which inlet the expedition sailed beyond 81° of west longitude in the parallel of 74° and a few minutes.

It is much to be regretted that the service did not admit an opportunity to be afforded, of making observations on the various magnetic phænomena, with the excellent instruments supplied to the expedition, at this very interesting place; where a nearer approach was made to one of the magnetic poles than had ever been known before.

But in the absence of any actual observation on the dip of the needle, this fact of the error of the compasses having increased from local attraction so greatly beyond the amount which had been before observed, is worthy of notice, as affording an indication that the dip had also increased, and not inconsiderably. The greatest which was observed, was $86^{\circ} 09'$; and after this observation, the ships continued to sail for six days in the direction in which the dip had hitherto been found to increase.

In concluding this paper, it may be permitted to remark, that it is to the voyages of discovery undertaken during the reign of his present Majesty, that a knowledge of the extent and causes of the errors to which a compass is subject in ships, is to be principally attributed; as well as the steps that have been taken towards the investigation and remedy of the inconvenience they occasion to practical navigation.

The care and exertions of Captain Flinders in collecting observations for this purpose, give his opinions and rules a peculiar claim to attentive consideration. No one could have been more fully persuaded than he was, that a rule, founded on the effects experienced in a few ships, would require a far more extensive trial, before it could be depended on for general application.

To carry this on, therefore, is to follow his useful example, and to effect what he was desirous to have done himself, had his life been spared.

XLVI. *On the Anomaly in the Variation of the Magnetic Needle as observed on Ship-board.* By WILLIAM SCORESBY Jun., Esq. Communicated by the Right Hon. Sir JOSEPH BANKS, Bart. G.C.B. P.R.S.*

THE anomalies discovered in magnetical observations conducted on ship-board, were usually attributed to the imperfection of the azimuth compass, until Captain Flinders, in his modest and enlightened paper on this subject, published in the Philosophical

* From Philosophical Transactions of the Royal Society for 1819, Part I.
Transactions,

Transactions, suggested that they were probably owing to the concentration of the magnetic influence of the iron made use of in the construction of the ship. The truth of this suggestion, and the accuracy of his observations, have since met with full confirmation, and his practical rules founded thereon have received additional support, from the "Essay" of Mr. Bain "on the Variation of the Compass," published last year.

As I have been materially anticipated by Mr. Bain in a series of observations on the variation of the compass*, which I conducted on the coast of Spitzbergen, in the years 1815 and 1817, it will be unnecessary here to enter into the detail of these observations, or enlarge upon the probable cause of the anomalies observed; it may be sufficient to give a table of the most accurate of my observations, and annex to it the few general inferences which were drawn from it, during the voyage in which most of the observations were made, together with such remarks on each inference as seemed to me calculated for its elucidation. I shall however just premise, that I am not unconscious of the great liability to error in observations of this kind, and of the variety of causes (arising out of the unequal distribution of iron in different ships, whereby numerous local attractions are formed) which contribute to the multiplication of those errors: it is, therefore, with the greatest deference that I submit these deductions, particularly as I conceive it will require observations to be made under a vast variety of circumstances, and in many different vessels, before *correct* and satisfactory conclusions can be drawn. It is *only* then as a step towards facilitating such general conclusions, the importance of which to our maritime concerns is so obvious, that I presume to offer these observations and remarks.

* The azimuths contained in the following table were taken, either by the needle of a theodolite, or by a compass fitted up at sea, for the purpose, with a card made extremely light, and a bar fastened edgewise to it, by two brass screws, *a a*, as in the annexed sketch. The compass being small, the card light, and the needle very powerful, owing to the thickness of its ends†, it performed considerably better than an expensive azimuth compass of larger dimensions, which indeed was so sluggish and erroneous in its indications, that I could make no good use of it.

† See Plate IV. fig. 3.

Table of Magnetical Observations made on board of the Ship *Esk*, in view to investigate the Laws by which the Anomalies, or Errors, board are regulated.

No.	Date.	Latitude.	Longitude.	Time. [Ch. Chrono- meter.] [Ap. Appa- rent.]	Sun's Azimuth.		Variation.	
					Observed.	True.	Apparent or observed.	Supposed true.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
1	1815. June 30	77° 19' N.	12° 15' E.	Ap. Noon.	S. 7° W.	South.	7° W.	21° W.
2					38	South.	38	21
3					21	South.	21	21
4	1817. June 6	77 56	0 15 W.	Ch. 6 ^h 54' P.M.	N. 36 W.	N. 73° W.	37	32° 30
5				7 28	35½	64 30	28° 45	32° 30
6				7 28	45	64 30	19° 30	32° 30
7				6 54	27½	73 0	45° 30	32° 30
8				7 32	33	63 30	30° 30	32° 30
9				7 33	31	63 15	32° 15	32° 30
10	June 19	77 36	2 35 W.	Ap. 11 33	S. 25 W.	S. 5 30 E.	30° 30	36° 0
11				11 20	27	10 0	37° 0	36° 0
12				12 3	34½	S. 0 45 W.	34° 0	36° 0
13	— 28	76 24	4 45 W.	Ch. 7 34	N. 46 W.	N. 68 11 W.	22.11	37° 0
14				7 38	16½	67 11	50° 41	37° 0
15	July 16	76 6	0 30 W.	Ch. 6 33	60	79 54	19° 54	33° 0
16				6 36	33	79 9	46° 9	33° 0
17				6 31	46	80 24	34° 24	33° 0
18				6 44	53	77 9	24° 9	33° 0
19				6 43	41	77 24	36° 24	33° 0
20				6 41	70	77 54	7° 54	33° 0

Voyages to the Greenland or Spitzbergen Whale Fishery; with a discovered in the Indications of the Mariner's Compass on Ship-

Errors produced in the observed variation by the attraction of the ship on compass needle.			Ship's head by binnacle compass.	Situation of the compass by which the Sun's azimuth was observed. [col. vi.]
Errors attributed to the position of the ship's head. [Diff. of col. viii. and ix.]	Differences produced by a change in the position of the ship's head. [From col. viii.]	Differences produced by a change in the position of the compass. [From col. viii.]		
X.	XI.	XII.	XIII.	
14° 0' } 17 0 } 0 0 }		31° 0' } 17 0 }	S. by E. } S. by E. }	Starboard side of the main deck. Larboard ditto.
4 30 } 3 45 }	8° 15'		W. $\frac{1}{2}$ S. } East. }	On the binnacle amidships of the quarter-deck, 10 ft. from taffrail.
13 0 }		9 15 }	East. }	Compass on a stand on the middle of the quarter-deck, 6 feet abaft capstern spindle.
13 0 } 2 0 }	26 0		W. $\frac{1}{2}$ S. } E. $\frac{1}{4}$ S.	Starboard side of the main deck.
0 15 } 5 30 }		1 45 }	E. $\frac{1}{2}$ S. } E. by S.	Larboard side of the main deck. On the binnacle, 10 ft. from taffrail.
1 0 } 2 0 }	6 30 3 0		N. }	Ditto.
14 49 }		28 30 }	NE. $\frac{1}{2}$ E. } NNE.	Ditto. Starboard side of the main deck.
13 41 } 13 6 }		26 15 }	NN.E. } South.	Larboard ditto. Starboard side of the main deck.
13 9 }		11 45 }	South. }	Larboard ditto.
1 24 } 8 51 }			South. }	Centre of main hatches. Centre of fore-hatches.
3 24 } 25 6 }		12 15 } 28 30 }	South. }	Larboard side fore-castle near the windlass end. Starboard ditto.

From these observations, and from the assistance afforded by the lucid remarks of Captain Flinders, the inferences which follow are deduced.

1. In the construction of every ship, a large quantity of iron being used, the portions thereof which have a perpendicular position, such as standard and hanging knees, the nails and bolts in the deck, the capstern spindle, flukes of the anchors (when at sea), chain-plates, iron stanchions and riders; the eye bolts, transom bolts, joint bolts, &c. of gun carriages, and possibly the upper surfaces of the guns themselves, &c. &c. have a tendency to become magnetical, the upper ends being *south* poles and the lower *north* poles, in this hemisphere, where the *north* end of the needle dips, but the contrary in the southern hemisphere, where the *south* end of the needle dips.

2. The combined influence of the iron distributed through all parts of the ship, seems to be concentrated into a kind of magnetic *focus of attraction*, the principal south pole of which being upward in the northern hemisphere, is probably situated, in general, near the middle of the upper deck, but nearer to the stem than the stern.

Wrought iron having a much greater attraction for the magnetic needle than cast-iron, the anchors, which usually lie about the bows, possess much more influence than guns; hence, the focus of attraction lies nearer to the bows than to the stern.

3. This focus of attraction so influences the compass needle, that it is subject to an *anomaly*, or variation from the true meridian, different from what is observed by a compass on shore; the north point of the compass being constantly drawn towards the focus of attraction, which appears to be a south pole in north dip; and the south point being attracted in south dip, where the focus of attraction probably becomes a north pole.

The phenomenon of a ship appearing to lie nearer the wind when beating to the northward, with the wind at north, than when beating to the southward, with a southerly wind, was observed by my father at least 20 years ago, which phenomenon he attributed to the "attraction of the ship upon the compass;" and ever since the year 1805, I have been in the habit of allowing only 2 to $2\frac{1}{2}$ points variation on the passage outward to Greenland, with a northerly or north-easterly course, but generally three points variation on the homeward passage when the course steered was S.W. or S.W. b. W. Without this difference of allowance, a Greenland ship outward bound will be generally found to be to the eastward of the reckoning, and homeward bound will be even 4 or 5 degrees to the eastward of it.

4. This anomaly in the variation of the compass, occasioned by the attraction of the iron in the ship, is liable to change with every

every alteration in the dip of the needle, in the position of the compass, or in the direction of the ship's head.

If the intensity of the terrestrial magnetism be not equal in all parts of the globe, then the anomaly in the variation of the compass will be also liable to change with every alteration in the magnetic influence of the earth. This is a point of such importance, I conceive, in the science of magnetism, that I was very anxious to procure a dipping needle on my last voyage to Greenland, to ascertain whether the magnetism of the earth, by which the dipping needle is influenced, be not greater near the magnetic pole than it is in England. If it be equal, the oscillations of the same dipping needle would be performed, circumstances as to temperature and "local attraction" being the same, in equal spaces of time in both places; but if the magnetic power in either place be greater, the oscillations of the needle would there be quicker. The number of vibrations of a horizontal needle, performed in a certain space of time in Greenland, is to the number performed in an equal space of time in England as 5 to 6, each longer vibration in England being performed in five seconds, and in Greenland in six. No alteration was observed in the time required for each vibration, whether the temperature was high or low, but I think in a low temperature the vibrations performed by the needle before it stopped were fewer.

5. The anomaly of variation bears a certain proportion to the dip of the needle, being greatest where the dip is greatest, diminishing as the dip decreases, and disappearing altogether on the magnetic equator.

Captain Flinders ascertained, that the medium error or anomaly for 8 points deviation of the Investigator's head, on either side of the magnetic meridian, was very nearly $\frac{1}{20}$ of the dip, $\cdot 05$ the decimal expression of which he considered to be the common multiplier to the dip, for obtaining the radius of error at any situation in the southern hemisphere; and $\cdot 053$ to be the common multiplier, from England to the magnetic equator. This, however, can only be correct within certain limits, as on the magnetic pole, where the anomaly would probably be equal to the dip, or 90° , the decimal multiplier would require to be increased to $1\cdot 0$. Hence it has been suggested, by an officer on board one of the vessels now in search of a north-west passage, that in those parts of the globe where the dip is 90° , the compass needle would probably always stand N. and S., by the attraction of the ship. This position clearly follows from the inference above, provided the compass be placed near the ship's stern in midships; but if placed as described in inference No. 8, the ship's head by the compass on the starboard side of the main deck would always appear to be *east*, and on the larboard side *west*.

6. A compass placed near the stern, amidships of the quarter-deck, is subject to the greatest anomaly or deflection from the magnetical meridian, when the ship's course is about west or east; because the focus of attraction then operates at right angles to the position of the compass needle; but the anomaly disappears when the course is about north or south, because the focus of attraction is then in a line with, or parallel to, the compass needle, and consequently has no power to deflect it from its direct position. [See Observations No. 4, 10, 11, and 12 of the prefixed table.]

This situation for the *binnacle* is deemed one of the best in the ship, and is very properly preferred. Being abaft the focus of attraction, the north point of the compass, in this magnetic hemisphere, is always attracted forward, and the errors at equal distances from the magnetic meridian, in the same dip, are alike in quantity both on easterly and westerly courses, and always towards the north; the correction, when applied to the apparent course, must therefore be towards the south, to give the true course steered. Thus in high northern latitudes, where the anomaly is great, (say 20° , or 10 degrees on each side of the magnetic meridian,) a ship steering west by the compass 100 leagues, and then east 100 leagues, instead of coming to the place from whence she started, will be 104 miles to the southward of it.

7. The greatest anomaly with the compass in the position last described, being ascertained by observation, the error on every other point of the compass may be easily calculated; the anomalies produced by the attraction of the iron in the ship, being found to be proportionate to the sines of the angles between the ship's head and the magnetic meridian.

Captain Flinders's rule is—As the sine of eight points (or radius) is to the sine of the angle between the ship's head and the magnetic meridian (or sine of the course reckoned from south or north), so is the anomaly found at east or west by observation, to the anomaly on the course steered; or, the anomaly on any other course being found by observation, the error on that position of the ship's head "would be to the error at east or west, at the same dip, as the sine of the angle between the ship's head and the magnetic meridian, to the sine of eight points, or radius."

8. A compass placed on either side of the ship's deck, directly opposite to, or abreast of, the focus of attraction, gives a correct indication on an east or west course, but is subject to the greatest anomaly when the ship's head is north or south; and being here nearer the focus of attraction, the anomaly is much greater than that observed on an east or west course with the compass placed in the binnacle near the ship's stern.

This inference is founded on Observations No. 1, 2, 3, 8, 9,
13,

13, 14, 15, 16 and 17, of the prefixed table. The latter part of the inference, namely, that the greatest anomaly occurs here when the ship's head is north or south, is fully and uniformly established; but the former part rests only on the authority of Observations No. 8 and 9, though it derived additional support from several observations which I have excluded; because neither the sun, nor any other distant object, calculated for proving the accuracy of the observations and determining the clear effect of the "local attraction," was visible.

9. A compass placed within six or eight feet of a capstern spindle, or anchor, or other large mass of wrought iron, forgoes, in a great measure, the influence of the focus of attraction, and submits to that of the nearer body of iron.

The effect of this is various, according to the relative position of the compass and the iron. When the compass is placed directly *abast* the body of iron, the influence is similar to, but greater than, that of the focus of attraction on a compass placed near the stern, as described in inference No. 6. [See Table of observations prefixed, No. 6 and 7.] When placed directly *before* it, the anomaly is similar in quantity, but has its sign reversed; and when placed on either side of the mass of iron, the influence corresponds more nearly with that of the focus of attraction on a compass placed in the sides of the ship opposite to it, as described in inference No. 8. A compass placed upon the *drum head* of the capstern, any where out of the centre, will have its north point so forcibly attracted by the upper end or south pole of the spindle, that the ship's head may be made to appear to be directed to any point whatever, at the pleasure of the experimenter. I have sometimes excited the astonishment of my officers by taking the binnacle compass and so placing it on the capstern head, that the ship has appeared to be steering a course directly contrary to that intended.

10. When the iron in a ship is pretty equally distributed throughout both sides, so that the focus of attraction occurs in midships, a compass placed on the midship line of the deck (drawn longitudinally) will be free from any anomaly from one end of the ship to the other, when the course is north or south; but on every other course an anomaly will generally appear, increasing as the angle between the ship's head and the magnetic meridian increases, until the error is at a maximum, when the course is east or west.

The unequal distribution of iron in the ship, on board of which I made all my experiments, prevented the above effect from being realized. A blacksmith's shop was situated between decks, on the larboard side of the fore hatchway. It was lined with

sheet iron; and besides the armourer's forge, vice, &c. contained a large quantity of other iron. The effect of this, together with the anchors, windlass necks, and other iron, was very remarkable on a compass placed in different parts of the deck near the foremast. [See Observations 18, 19, and 20 of the prefixed Table.]

11. As a compass placed on the midship line of the deck is subject to no anomaly fore and aft, in certain ships, on a north or south course [Inference No. 10], and as a compass in either side of the ship, opposite to the focus of attraction, shows no anomaly on a west or east course [Inference No. 8], the intersection of the line joining the two situations in opposite sides of the ship with the midship line traced fore and aft, will probably point out a situation directly over the top of the focus of attraction, where no anomaly on any course whatever will appear.

The *Esk*, in which I made my magnetical observations, had, as above stated, an armourer's forge near the larboard bow, which with the varying position of large quantities of iron work, composing our whale-fishing apparatus, contributed to vary this point where no anomaly is supposed to exist, and prevented me from ascertaining satisfactorily, at any time, its precise situation. I made indeed but very few observations with this view, and these I find neither establish nor refute the inference.

12. The anomaly of variation is probably the greatest in men of war, and in ships which contain large quantities of iron; but it exists in a very considerable degree also in merchantmen, where iron forms no part of the cargo, especially in high latitudes, where the dip of the needle is great.

Whitby, 3d Nov. 1818.

WILLIAM SCORESBY Jun.

XLVII. *Further Remarks on the Mode of taking Lunar Observations.* By Mr. HENRY MEIKLE.

To Mr. Tilloch.

SIR,—IN my paper on the lunar observations, a property of the ellipsis was referred to, which I suspect is hardly to be met with in an elementary treatise, if indeed in any other work, and shall therefore now give a demonstration of it in a more general proposition.

Theorem—The normal drawn from the transverse axis of an ellipsis is less than the semiaxis minor, when they do not coincide; but the normal produced to meet the conjugate axis is greater than the semiaxis major not coinciding with it; and the normal and normal so produced are in the duplicate ratio of the axes.

Let

Let AHB be an ellipsis, and GH the normal or perpendicular to the tangent KP which touches the curve in H. From F and L, the foci, draw FP, LK perpendicular to KP; join FH, LH.

Now it is a known property of the ellipsis, that the angle FHP = LHK, or that the triangles PFH, KLH are similar. Draw GQ, LR parallel to KP, and the triangles GLR, FGQ will also be similar.

Whence $PF - HG : HG - KL :: PH : HK :: FP : KL$, and

$HG = \frac{2FP \times KL}{FP + KL} = \frac{2CD^2}{FP + KL}$. But when H and E do not coincide, $KL + FP$ is greater than $2CD$, because CD^2 is known to be equal $KL \times FP$. Consequently, HG is less than CD the semi-axis minor.

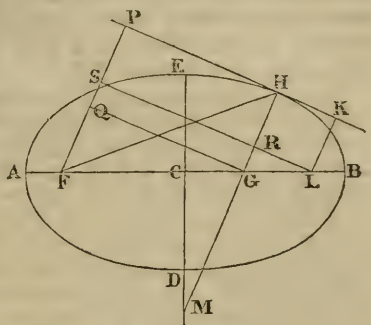
Again: Since FP , GH and LK are parallel, and the triangles FHP, LHK similar, we have $FP + KL : FP - KL :: KP : HP - HK :: FL : 2CG$. Produce LR to meet FP in S , and HG to meet ED in M . Then the triangles LFS, MGC will also be similar; for since MG is parallel to FS , the angle $LFS = MGC$, and the angles FSL , GCM are likewise equal, being right angles. Hence $FP - KL : FL :: CG : GM$. Consequently $FP + KL : FL ::$

$\frac{1}{2}FL : GM = \frac{\frac{1}{2}FL^2}{FP + KL} = \frac{2CL^2}{FP + KL}$; and $MH = MG + GH = \frac{2CD^2 + 2CL^2}{FP + KL} = \frac{2BC^2}{FP + KL}$. But when H and B do not coincide,

$FP + LK$ is less than $FH + HL = AB = 2BC$, because the side of a right-angled triangle is less than the hypotenuse. MH is therefore greater than BC . It is hence evident that $GH : HM :: CD^2 : BC^2 :: DE^2 : AB^2$, which is a very elegant property of the ellipsis.

When H comes to E, $HG = CD$, and $HM = \frac{BC^2}{CD}$ the semiparameter of the axis minor, which is also the radius of curvature at E; and when H and B coincide, $MH = BC$, and $GH = \frac{CD^2}{BC}$ the semiparameter of the axis major, which is likewise the radius of curvature at B.

It might likewise be shown conversely, that if a variable straight



straight line MH be divided in G, so that HM may be to HG in a constant ratio, and revolve so as to keep M and G respectively in DE and AB, two fixed straight lines crossing at right angles, and also that MH be always perpendicular to the path H describes: then shall the *locus* of H be an ellipsis, whose axes are in the ratio of $\sqrt{HG}$ to $\sqrt{MH}$.

The principle of the elliptical compasses is much simpler than this; and differs from it, in that the bar to which the tracing point is attached is never perpendicular to the curve, but at the vertices of the axes; and also that the distances of the sliding dovetails from the tracer are equal the semiaxes.

A correspondent to the *Annals of Philosophy* lately objected to periodical mathematical works, for not admitting any thing unaccompanied by demonstration; and in effect proposed that demonstration be done away with in future. This, no doubt, he did for the same reason that smugglers wish to be disencumbered of "those oppressive laws," hoping thereby to get off his contraband doctrines with more facility. I cannot, however, second his motion, on the ground that no new doctrine on which any stress is laid can ever be safely let loose into the world without some evidence of its truth. It is this which makes the grand distinction between true science and unmeaning jargon. Things, it is true, which have often been demonstrated, may be quoted or given without demonstration. But many erroneous propositions are copied from book to book, without the least suspicion of their accuracy. A remarkable instance of this is the "Construction of the dialling lines," which, though admitted into our first-rate works of science, is nevertheless grossly erroneous. It does not appear that an absurdity becomes more correct by frequent copying, but it certainly becomes more dangerous.

Periodical mathematical works, however, are liable to a more serious charge than the one above mentioned; in that they are mostly made up of mere puzzling questions, totally useless and unconnected with science. Persons who torment themselves with such nonsense, might as well be assisting Sisyphus in rolling his stone, for any good they are doing either themselves or others, except it be to keep them out of mischief. But even this laudable end might be attained in solving questions serving some more useful purpose. I am, sir, your most obedient servant,

Berners Street, Oct. 1, 1819.

HENRY MEIKLE.

P.S.—In concluding the last paper on friction, I made a remark on a position which I then thought inconsistent with the case under consideration. The truth however is, that it is not simply so, but impossible that bodies of different weights with equal bases can make impressions proportional to their weights in

in equal times; because the lighter body will always be found to perform its task in the shorter time, if the resistance increase as any single power of the depth. This, if necessary, shall be demonstrated afterward. The statement referred to, therefore, does not imply any possible law of resistance. H. M.

XLVIII. *Illustrations and Corrections of two Papers on the Nature and Laws of Friction, with a Refutation of the Objections of Mr. MEIKLE. By Mr. THOMAS TREDGOLD.*

To Mr. Tilloch.

SIR, — IN your last Number Mr. Meikle has made some objections to a theory of Friction, which you did me the favour to publish in your Numbers for January and July. I hope in this letter to show that those objections are groundless; but though Mr. Meikle states that he has examined the steps of my inquiry, the real errors he has failed in detecting; and I am sorry to say that in two cases I have overlooked circumstances that ought to have been considered: therefore I take this opportunity of correcting them.

In your Number for January, page 5, it is stated that the indentation is as the extensibility; but it ought to have been directly as the extensibility, and inversely as the cohesive force:

That is, the general Prop. (2) ought to have been $I : \frac{P \times E}{C \times L \times B}$.

And then, Prop. (4) becomes $F : P \times E$; that is, the friction is directly as the pressure and extensibility and independent of the cohesive force. This correction is important, in as far as relates to the friction of different bodies, but does not affect the results when the materials continue the same. I am sorry that the effect of the cohesive force in resisting indentation was overlooked: the only reparation I can now make is to candidly avow it.

I must now proceed to examine Mr. Meikle's objections. In the first place, he states that my law of friction in uniform motions is in opposition to several experiments. Now it certainly must have been in your correspondent's power, and therefore it was a duty he had imposed upon himself, to refer to the experiments he alludes to; and to show that they were of an unobjectionable nature. As he has not done so, I can only say that such experiments are unknown to me.

It has been supposed that the friction is the same in uniform motions as it is in accelerated ones; but, independent of mathematical reasoning, the incorrectness of this supposition must be apparent when the distinct laws of these motions are considered. In the description of experiments, and in the usual statements of

the phenomena of friction, the kinds of motion are seldom, if ever, distinguished; and we are left to conclude that it is indifferent whether the motion be uniform or accelerated. I hope I have been successful in showing that there is a material difference; and it appears to me that the distinction will be of much use in many of the most important physico-mathematical researches.

Your correspondent further states, that the indentation cannot be as the square of the time; I know it is not correctly so; but from the following investigation it will appear that it is a very near approximation to the truth.

Let W = the weight or pressure producing the indentation;
 d = the depth of indentation when the resistance of the pressed surface is exactly equal to the weight W ;
 and x = any variable depth counted from the surface.

Then, as the indentation is always proportional to the pressure,

$$d : x :: W : \frac{Wx}{d} = \text{the resistance at the depth } x.$$

$$\text{Hence } \frac{W - \frac{Wx}{d}}{W} = 1 - \frac{x}{d} = \text{the accelerative force.}$$

$$\text{By the laws of variable forces } vv = 2g\dot{x} = 2g\left(1 - \frac{x}{d}\right)\dot{x}.$$

$$\text{Taking the fluents, } v^2 = 4g\left(x - \frac{x^2}{2d}\right); \text{ or } v = \sqrt{\frac{2g}{d}} \times \sqrt{2dx - x^2}.$$

When $x = d$, $v = \sqrt{2gd}$. And it appears that at the depth d the accelerative force is nothing; for then $x = d$, and $1 - \frac{d}{d} = 0$.

A retarding force then commences, and the body will descend till this retarding force destroys the whole of the velocity generated in moving through the space d . When the whole of the velocity is destroyed the body will ascend, and after several vibrations will ultimately rest at the depth d .

$$\text{Let } t \text{ be the time of descent, then } t = \frac{\dot{x}}{v} = \frac{1}{\sqrt{\frac{2g}{d}}} \times \frac{\dot{x}}{\sqrt{2dx - x^2}}.$$

The fluent may be found by means of a circular arc, but for the present purpose it is perhaps better to express it in a series.

In that case we have

$$t = 2\sqrt{gx} \times \left(2 + \frac{x}{6d} + \frac{3x^2}{80d^2} + \frac{5x^3}{448d^3} + \&c.\right); \text{ or}$$

$$t^2 = 4gx \times \left(2 + \frac{x}{6d} + \frac{3x^2}{80d^2} + \frac{5x^3}{448d^3} + \&c.\right)^2. \text{ Hence it ap-}$$

pears that the indentation increases very nearly as the square of the time; and that the proportion $x : t^2$ which I have used is sufficiently near the truth for practical purposes.

By taking the value of d such that the resistance is exactly equal

equal to the pressure of the weight W , the resulting equations show more clearly the circumstances which have place during the descent, and also point out another correction. For the depth of descent, x , does not increase directly as the force, and inversely as area and the modulus of elasticity. Let a =the area, and m =the weight of the modulus of elasticity; then $d: \frac{W}{ma}$. Now when this value of d is substituted in the preceding equations, it is clear that the friction is not as the pressure, nor inversely as the area, except just when the body is first moved from rest. Consequently, these principles again lead to results which Professor Vince had ascertained by experiments*; viz. that the friction of a body in motion increases with the area of the body, and in a less ratio than the pressure.

Mr. Meikle next alludes to some experiments of Mr. Rennie's, but I have not seen any of his experiments that have the least connexion with the subject.

When Mr. M. recollects that the time is a given quantity, or, in other words, that the quantity of friction produced in a given time is the thing to be estimated, his confused notion of the past and present part of a given time will perhaps be removed.

And lastly, I think it will appear very clear to your correspondent, that I have not committed an error in estimating the friction at the axis of a wheel, when he considers that the space moved through increases with the radius of the axle as well as the velocity of the moving parts; therefore, enlarging or diminishing the axis does not affect the resistance of the rubbing surfaces, and in this respect my conclusion remains correct.

It may be necessary to remark, that the results that I have obtained are not contradictory; they are deduced from the known laws of the cohesive force and extensibility of bodies, and the more minutely they are investigated the more correctly they agree with experiments. It will also be recollected, that it was my object to obtain simple practical rules; and that I did not profess to give an accurate analysis of the laws of Friction. The method I have adopted is simple, and may be easily followed by those who are acquainted with the mechanical properties of matter and their laws of action; but it is useless for any one to offer random objections to any thing they do not understand.

I am, Sir, yours, &c.

THOMAS TREDGOLD.

* Philosophical Magazine, vol. xvii. p. 47.

XLIX. *Some Observations on the Formation of Mists in particular Situations. By Sir H. DAVY, Bart. F.R.S. V.P.R.I.**

ALL persons who have been accustomed to the observation of Nature, must have frequently witnessed the formation of mists over the beds of rivers and lakes in calm and clear weather after sun-set; and whoever has considered these phenomena in relation to the radiation and communication of heat and nature of vapour, since the publication of the researches of MM. Rumford, Leslie, Dalton, and Wells, can hardly have failed to discover the true cause of them. As, however, I am not aware that any work has yet been published in which this cause is fully discussed, and as it involves rather complicated principles, I shall make no apology for offering a few remarks on the subject to the Royal Society.

As soon as the sun has disappeared from any part of the globe, the surface begins to lose heat by radiation, and in greater proportions as the sky is clearer; but the land and water are cooled by this operation in a very different manner: the impression of cooling on the land is limited to the surface, and very slowly transmitted to the interior; whereas in water above 45° Fahrenheit, as soon as the upper stratum is cooled, whether by radiation or evaporation, it sinks in the mass of fluid, and its place is supplied by warmer water from below, and till the temperature of the whole mass is reduced nearly to 40° F. the surface cannot be the coolest part. It follows, therefore, that wherever water exists in considerable masses, and has a temperature nearly equal to that of the land, or only a few degrees below it, and above 45° F. at sun-set, its surface during the night, in calm and clear weather, will be warmer than that of the contiguous land; and the air above the land will necessarily be cooler than that above the water; and when they both contain their due proportion of aqueous vapour, and the situation of the ground is such as to permit the cold air from the land to mix with the warmer air above the water, mist or fog will be the result; which will be so much the greater in quantity, as the land surrounding or inclosing the water is higher, the water deeper, and the temperature of the water, which will coincide with the quantity or strength of vapour in the air above it, greater.

I shall detail some observations which appear to me to show the correctness of this view. June 9th, 10th, 11th, the temperature of the atmosphere and of the Danube was repeatedly examined during a voyage that I made upon this river from Ratisbonne to Vienna, and on each of these days, the sky being per-

* From Philosophical Transactions for 1819, Part I.

fectly clear, the appearance of mist above the river in the evening uniformly coincided with the diminution of the temperature of the air from three to six degrees *below* that of the river, and the disappearance of fog in the morning with the elevation of the temperature of the air *above* that of the river. From Ratisbonne to Passau, the temperature of the Danube was pretty uniform throughout the 24 hours, being highest, 62° F. or $62\frac{1}{4}^{\circ}$ F., between 12 and 2 o'clock, and about one degree less before sun-rise, and the temperature of the air from 61° F. to 73° F. during the day, and from 61° to 54° F. during the night. Below Passau, the Inn and the Ilz flow into the Danube*. On examining the temperature of these rivers at 6 o'clock A.M. June 11, that of the Danube was found to be 62° F., that of the Inn $56\frac{1}{2}^{\circ}$ F., and that of the Ilz 56° F.: the temperature of the atmosphere on the banks where their streams mixed, was 54° . The whole surface of the Danube was covered with a thick fog; on the Inn there was a slight mist, and on the Ilz barely a haziness, indicating the deposition of a very small quantity of water. About 100 yards below the place where the three rivers joined, the temperature of the central part of the Danube was 59° F., and here the quantity of mist was less than on the bed of the Danube before the junction; but about half a mile below, the warmer water had again found its place at the surface, and the mist was as copious as before the union of the three rivers. June 12th, the evening was cloudy, and the temperature of the atmosphere remained till after dark higher than that of the river, being, when the last observation was made, 63° F. when there was not the slightest appearance of mist. The sky was clearer before sun-rise on the 13th, and the thermometer immediately after sun-rise, in the air above the river, stood at $55\frac{1}{2}^{\circ}$ F. the temperature of the Danube being 61° F.; a thin mist was seen immediately above the river; but there being no mass of vapour to exclude the sun-beams, it rapidly disappeared, and was not visible a few feet from the surface; and in half an hour the whole atmosphere was perfectly transparent.

In passing along the Rhine from Cologne to Coblenz, May 31st and June 2d and 3d, the nights being very clear, the same phenomenon of the formation of mists was observed, precisely under the same circumstances; but as I could examine the temperature of the air and of the river only close to the banks, and in two or three situations, my observations were less precise and less numerous; the mist formed later in the evening, and disappeared sooner in the morning, than on the Danube; which was evidently owing to the circumstances of the atmosphere being

* * The Danube was greenish, the Inn had a milky blueness, the Ilz was perfectly pellucid; but from the rapidity with which the Inn descended, its waters at this spot give their tint to the whole surface.

warmer and the river colder, the temperature of the one being from 66° F. to 75° F. during the day, and that of the river, where I examined it, from 59° to 60° F.

July 11th. I examined the temperature of the Raab near Kermond in Hungary, at 7 o'clock P.M. and found it 65° F. that of the atmosphere being 72° F. During the whole evening there were some thin fleecy clouds in the western sky, which being lighted up by the setting sun, greatly interfered with the cooling by radiation from the earth, and at half past nine the thermometer, in the atmosphere, was still 69° F. and at half past ten 67° F. when there was not the slightest appearance of mist. In the morning, before sun-rise, the temperature of the atmosphere on the banks was 61° F., that of the river 64° F., and now the bed of the river was filled with a white thin mist, which entirely disappeared half an hour after sun-rise.

I made similar observations on the Save in Carniola, in the end of August; on the Isonzo in the Friul, in the middle of September; on the Po near Ferrara, in the end of September; and repeatedly on the Tiber and on the small lakes in the Campagna of Rome in the beginning of October; and I have never in any instance observed the formation of mist on a river or lake, when the temperature of the water has been lower than that of the atmosphere, even when the atmosphere was saturated with vapour.

It might at first view be supposed, that whether the cooling cause existed in the water or the land, the same consequences ought to result; but the peculiar properties of water, to which I referred in the beginning of the paper, render this impossible. Water in abstracting heat from the atmosphere becomes lighter, and the warmer stratum rests on the surface, and its operation in cooling the atmosphere is extremely slow: besides, the cooled atmospheric stratum remains in contact with it, and water cannot be deposited from vapour, when that vapour is rising into an atmosphere of a higher temperature than its own; and the law holds good, however great the difference of temperature. Thus, August 26, at sun-set, the day after a heavy fall of rain, and when the atmosphere was exceedingly moist, I ascertained the temperature of the Drave near Spital in Carinthia, and though it was 14° F. below that of the air, yet the atmosphere above the river was perfectly transparent.

It may be imagined, that without any reference to the cooling agencies of air from the land, mist may form upon rivers and lakes, merely from the loss of heat by radiation from the air, or the vapour itself immediately above the water; and that the phenomenon is merely one of the formation of vapour, the source of heat being in the water; and its deposition, the source of cold, being in the atmosphere; but it is extremely improbable, that air or in-
visible

visible vapour, at common temperatures, can lose any considerable quantity of heat by radiation ; and, if mist could be formed from such a source, it must always be produced to a great extent upon the ocean in calm weather during the night, particularly under the line, and between the tropics, which the journals of voyages sufficiently prove is not the case. I have myself had an opportunity of making some observations which coincide with this view. During a voyage to and from Pola, I passed the nights of September 3, 5, and 6, off the coast of Istria ; there was very little wind on either of the nights, and from sun-set till nearly midnight it was perfectly calm in all of them. On the 3d it was cloudy, and the lightning was perceived from a distant thunder-storm, and the vessel was never far from the shore : but on the 5th and 6th the sky was perfectly clear, and the zodiacal light, after sun-set, wonderfully distinct and brilliant, particularly on the 5th, and we passed by the help of oars from two to eight miles from the shore. The temperature of the sea at sun-set was 76° F. on the 5th, 77° F. on the 6th, that of the atmosphere immediately above it 78° F. and 79° F. On the 5th, at midnight, about five miles from the shore, the temperature of the sea was 74° F. and that of the atmosphere 75° F., and on the 6th, at the same hour, at about four miles from the shore, the temperature of the sea was 73° F. and that of the atmosphere 75° F. There was not the slightest appearance of mist on either of these nights on the open sea, or at any distance from the land : but close under the hills of Istria there was a slight line of haze visible before sunrise, which was thickest under the highest land ; and as we approached at sun-rise, on the 7th, the mountains of the Friul, the tops of those nearest to Trieste were seen rising out of a thick white mist, which did not reach a quarter of a mile from the shore.

After mists have formed above rivers and lakes, their increase seems not only to depend upon the constant operation of the cause which originally produced them, but likewise upon the radiation of heat from the superficial particles of water composing the mist ; which produces a descending current of cold air in the very body of the mist, whilst the warm water continually sends up vapour : it is to these circumstances that the phænomena must be ascribed of mists from a river or lake, sometimes arising considerably above the surrounding hills. I have often witnessed this appearance during the month of October, after very still and very clear nights, in the Campagna of Rome above the Tiber, and on Monte Albano over the lakes existing in the ancient craters of this extinguished volcano ; and in one instance, on the 17th of October, before sun-rise, there not being a breath of wind, a dense white cloud

cloud of a pyramidal form was seen on the site of Alban lake, and rising far above the highest peak of the mountain, its form gradually changed after sun-rise, its apex first disappeared, and its body, as it were, melted away in the sun-beams.

Where rivers rise from great sources in the interior of rocks or strata, as they have the mean temperature of the climate, mists can rarely form upon them except in winter, or late in autumn, or early in spring. In passing across the Apennines, October 1st, 2d, and 3d, 1818, there having been much rain for some days preceding, and the nights being very clear, I observed the beds of all the rivers in the valleys filled with mist, morning and evening, except that of the Clitumnus near its source, in which there was no mist, and this river rises at once from a lime-stone bed, and when I examined it, at half past six o'clock A.M. October 3, was $7\frac{1}{2}^{\circ}$ lower than the atmosphere.

Great dryness in the air, or a current of dry air passing across a river, will prevent the formation of mist, even when the temperature of the water is much higher than that of the atmosphere: thus on the 14th of June, near Mautern, though the Danube at five in the morning was 61° F. and the air only 54° , yet there was no mist; but a strong easterly wind blew, and from the rapidity with which water evaporated it, it was evident that this wind was in a state of extreme dryness.

The Tiber has furnished me with a number of still more striking examples. October 13th, the night having been very clear, on arriving at the Ponte Molle, at half-past six in the morning, I found no mist on the river, yet the temperature of the air immediately above it was 48° F. and that of the river 56° F., a strong north wind blew, which indicated, by the hygrometer, a degree of dryness of 55° , and this part of the river was exposed to it; but the valley above, where the river was sheltered from the wind, was full of mist, and the mist in rising to the exposed level might be seen, as it were, dissolving, presenting thin striae which never reached above a certain elevation, and many of which disappeared a few seconds after they rose. From the 13th to the 25th of October, during which time the tramontane or north wind blew, I witnessed repeatedly the same phænomenon; and in the whole of this time there was only one morning when there was no mist in the sheltered valleys, and the cause was perfectly obvious; the night had been very cloudy, and the thermometer, before sun-rise, indicated a difference of only one degree in the atmosphere below that of the river.

It is not my intention to discuss the general subject of the deposition of water from the atmosphere, in this paper; but merely to describe a local cause of considerable extent and variety in its modifications:

modifications: and which is not without an effect in the œconomy of nature; for verdure and fertility, in hot climates, generally follow the courses of rivers, and, by the operation of this cause, are extended to the hills, and even to the plains surrounding their banks.

Rome, Dec. 8, 1818.

L. *Further Evidence to prove the Existence of the Kraken in the Ocean, and tending to show that this huge Creature is a Species of Sepia or Squid. Being three several Communications of Facts, made to Dr. MITCHILL, by WILLIAM LEE, Esq., Captain RILEY, and Captain NEVILLE, in September 1817. Communicated by Dr. MITCHILL.*

Copy of a Letter addressed to Dr. Mitchill by William Lee, Esq.

Washington, Sept. 2, 1817.

MY DEAR SIR,—THE description given in our newspapers of a sea serpent, lately seen for several days in and about Cape Anne Harbour, has brought to my recollection one of this species.

On a passage I made from Quebec, in 1787, in a schooner of about 80 tons burden, while standing in for the Gut of Canso, the Island of Cape Breton being about four leagues distant, one of the crew cried out “A shoal a-head!” The helm was immediately put down to tack ship, when, to our great astonishment, this shoal, as we thought it to be, moved off; and as it passed athwart the bow of our vessel, we discovered it to be an enormous sea-serpent, four times as long as the schooner. Its back was of a dark green colour, forming above the water a number of little hillocks, resembling a chain of hogsheads. I was then but a lad, and being much terrified, ran below until the monster was at some distance from us. I did not see his head distinctly; but those who did, after I had hid myself in the cabin, said it was as large as the small boat of the schooner. I recollect the tremendous ripple and noise he made in the water, as he went off from us, which I compared at the time to that occasioned by the launching of a ship.

My venerable friend Mr. ———, of your city, was a passenger with me at the time. He will corroborate this statement, and probably furnish you with a better description of this monster; for I well recollect his taking his stand at the bow of the vessel, with great courage, to examine it, while the other passengers were intent only on their own safety.

At Halifax, and on my return to Boston, when frequently describing this monster, I was laughed at so immoderately, that I found
it

it necessary to remain silent on the subject, to escape the imputation of using a traveller's privilege of dealing in the marvellous.

On the evening of September 9, Captain James Riley was at my house, and said he knew Captain Folger of Nantucket, who was occupied on a whaling voyage in the Southern Atlantic Ocean about 20 years ago. On the cruise, he saw an animal of uncommon size floating on the sea, off the coast of Brazil. Captain Folger then commanded a very large French-built ship, and the floating carcase was four or five times as long as his vessel. It attracted the spermaceti whales, who came to feed upon it, and had eaten away great portions of the flesh. He visited the huge body of the creature, and satisfied himself it was an enormous kraken. He hauled all his boats upon it, and his men ascended and lived upon it as if it had been a rock or island. They remained on it for the purpose of killing the whales that came to devour it. In this they were so successful, that by continuing there they took whales enough to load their vessel and complete her cargo. The back of the kraken was high and dry enough for them to inhabit temporarily, and to look out for their game. And when from this point of observation they discovered a whale coming to make a meal, they launched their boats from the top of the dead kraken, and made an easy prey of him. The substance of the monster's body was skinny, membranous and gelatinous, and destitute of the fat and blubber for which the whale is remarkable.

Captain Neville being on a voyage from London to Archangel, in the year 1803, saw floating on the ocean, in about the latitude of 68, a mass of solid matter of a dirty whitish colour, which, when he descried it, and for some time after, was believed to be an island of ice. On approaching it, however, he ascertained it to be an animal substance of an irregular figure, as if lacerated, decayed, and eaten away. The remnant of the carcase was nevertheless full as large as the brig in which he sailed; whose capacity was one hundred and eighty-nine tons, and length seventy feet. This enormous body was the food of animals both of the air and of the water; for as he sailed within a few rods of it, he saw great numbers of gulls and other sea fowl sitting on it and flying over it; those which were full retiring, and the hungry winging their way to it for a repast. He also beheld several cetaceous creatures swimming round it; some of them were whales of a prodigious magnitude, exceeding the vessel in length. Others were smaller, and seemed to belong to the grampus and porpoise tribe. He considered them all as regaling themselves with its flesh.

Near one extremity of this carcase, he distinguished an appendage

pendage or arm hanging down into the water, which, from his acquaintance with the sepia, he concluded to be that of the squid; being probably the only one left after the rest had putrified or been devoured. Such was likewise the opinion of a navigator, of much experience and long observation in the scenery of the north Atlantic, then on board; who remarked that the corrupting lump was intolerably fetid, and offensive to man; and would, if the brig was suffered to run against it, impregnate her with foulness and stench for the whole voyage. She was accordingly kept to windward for the purpose of avoiding it; but the smell was notwithstanding extremely nauseous and disgusting.

On conversing with mariners in the White Sea, such occurrences were spoken of by them, as too common to excite much attention or any doubt.

Afterwards while at Drontheim, in Norway, Captain Neville discoursed with practical men concerning things of this kind. The prevailing idea was, that such drifting lumps were by no means uncommon; that they were bodies or fragments of huge squids; that these were sometimes borne away by the Maelstrom current, and ingulfed and dashed to pieces by its whirlpools; and that these broken trunks and limbs were sometimes cast on shore, and sometimes tossed about on the sea.

It is supposed that squids and whales inhabit the same tracts of ocean; because the former furnish food for the latter, at least for the cachalats, orco, and other toothed and voracious species.

LI. *On the solid Excrement of the Boa Constrictor.* By EDMUND DAVY, *Professor of Chemistry and Secretary to the Cork Institution.*

A SERPENT of the genus *Boa*, the *Boa Constrictor*, was lately exhibited for some days in Cork. I had no opportunity of seeing it; but I have been informed, it was about 12 feet in length, very handsome, and quite domesticated. It was so gentle as to allow persons to touch it, and when permitted, it would twine itself round them and seem to enjoy the agreeable warmth of their clothes. In general, it was very inactive, except when stimulated by hunger, when it became more animated. Live rabbits were its usual food, one or more of which were given at intervals varying from about 10 to 21 days. When the serpent showed a disposition to eat, a rabbit was presented, which it first crushed, and then gradually swallowed without any previous mastication. It voided excrement about once a fortnight.

George Montague, esq. who had several opportunities of seeing this serpent, procured from its owner a portion of its excrement,

ment, part of which he very obligingly gave me. Not being aware that this substance had been analysed, I submitted it to such a chemical examination as the very limited quantity in my possession would allow. And as I presume there is some little novelty of result in my experiments, I beg leave to give you an account of them, with a view to their insertion in your valuable Magazine.

Properties and Composition of the solid Excrement.

1. This excrement when voided was soft, but it became hard by exposure to the air. In the state in which I saw it, it was rather larger than a pigeon's egg, of an amorphous form, and exhibited on its surface a number of irregular elevations and depressions. It was grayish white, and resembled chalk in colour and fracture; but it was more easily frangible than chalk, and its powder had a slight greasy feel. It had a slight odour, which was rather fragrant and agreeable. Its specific gravity could not differ much from that of chalk, for five grains of each in an impalpable powder occupied about the same space.

2. When heated by a spirit lamp on a slip of platinum, the excrement first became brown, and then black; it exhaled an ammoniacal smell, and the odour of decomposing animal matter. By keeping it at a red heat for a few minutes, it consumed without flame, leaving only a very minute grayish residuum, which deliquesced in the air, rendered turmeric brown, and dissolved with effervescence in muriatic acid. The muriatic solution was rendered blue by prussiate of potash, and gave with ammonia a little light coloured flocculent precipitate. These experiments seem to show that the minute portion of grayish residuum was composed of subcarbonate of soda, phosphate of lime, and oxide of iron.

3. The excrement was insoluble in water, in alcohol, and in muriatic acid, at a boiling heat. It dissolved slowly in sulphuric acid, with the evolution of minute globules of gas.

4. It readily dissolved with effervescence in strong nitric acid, the air disengaged appeared to be nitrous gas, as it produced red fumes on coming in contact with the atmosphere. Diluted nitric acid also dissolved the excrement, and a little gas was evolved. The nitric solution, when evaporated to dryness, assumed a fine pink colour; and on adding a little water, the fluid became of a carmine hue, and presently a reddish substance precipitated, leaving the fluid colourless. These experiments prove the existence of uric acid in the excrement, and the pink or reddish substance appeared to be the compound of purpuric acid and ammonia described by Dr. Prout*.

5. In an instance when five grains of the excrement were dis-

* Philosophical Transactions 1818.

solved in diluted nitric acid, and the heat continued for a short time after the solution had been boiled to dryness, besides the pink substance which lined the sides of the glass, there remained a yellow substance in appearance like bees wax. By submitting the excrement to destructive distillation, a yellow substance somewhat similar to the preceding sublimed in the neck of the retort. The yellow substance obtained by sublimation had at first an unpleasant smell, which it lost by exposure to the air for a few hours, and which arose from the presence of a little foetid oily matter. Its taste was pungent and peculiar. It was soft and easily frangible. It was volatile. It was soluble in water, in alcohol, in acids and in alkalies, by the agency of heat, but it was not precipitated from its alkaline solutions by muriatic acid. This yellow substance in a dry state did not affect moistened litmus paper, but when dissolved in warm water it reddened litmus. In most of its properties, this substance seemed to resemble Scheele's sublimate from the uric acid, described by Dr. Henry in his memoir on this acid*.

6. The fixed alkalies, in a concentrated state by the agency of heat, are capable of dissolving the excrement, and during the solution the odour of ammonia is exhaled. The alkaline solutions, when treated with muriatic or sulphuric acid, afford a white or yellowish white precipitate, which is either in small shining laminæ (in appearance like the benzoic acid), or in minute prismatic crystals. This precipitate has a soft greasy feel, is very slightly soluble in hot water, and dissolves readily and with effervescence in nitric acid. The nitric solution, when evaporated to dryness, leaves a yellow residuum, which when treated with a drop of ammonia instantly assumes a fine pink colour. Hence, the precipitate in question appears to be the uric acid. And in one experiment, in which I dissolved 10 grains of the excrement in pure potash, I obtained, by the agency of muriatic acid, one grain of uric acid.

7. The excrement by destructive distillation furnished a variety of products. The following results we obtained from 10 grains heated to redness for about a quarter of an hour, in a little retort over mercury, viz. about 6 cubic inches of gas; $1\frac{7}{8}$ grain of yellow sublimate (51); $1\frac{1}{2}$ grain of brownish sublimate impregnated with a foetid oily matter, and 3 grains of black coaly matter. The gas was not accurately analysed; but from a slight examination it appeared to be a mixture of nitrogen, ammonia, and carburetted hydrogen. It had a foetid ammoniacal odour, about one-sixth of it was readily absorbed by water, a small part of the residuum was inflammable, and the remainder extinguished a

* Memoirs of the Literary and Philosophical Society of Manchester, vol. ii.
Vol. 54. No. 258. Oct. 1819.

lighted taper. The 3 grains of coaly matter, on being kept at a red heat in a silver crucible for some time, slowly consumed, leaving only a residuum of $\frac{1}{16}$ of a grain, which on examination appeared to be subcarbonate of soda, phosphate of lime, and oxide of iron.

The preceding experiments seem to show that the excrement of the Boa is not very simple in its constitution. The uric acid appears to enter largely into its composition; and the facts, that ammonia is evolved from it by the fixed alkalies, and that the nitric solution of the excrement yields by evaporation the purpurate of ammonia, favour the idea that the volatile alkali also forms one of its ingredients. It also appears to contain a minute portion of subcarbonate of soda, phosphate of lime, and oxide of iron. From the limited quantities of the excrement examined, it would be rather premature to decide upon the products obtained from its destructive distillation; as a great part of them must necessarily arise from the decomposition of the uric acid in the process.

My cousin Dr. Davy, in his paper "On the urinary Organs and Secretions of some of the Amphibia*," states the solid urine of serpents to consist of nearly pure uric acid; and this solid urine seems closely to resemble the excrement of the Boa in its physical and chemical properties. But if my experiments are correct, the constitution of the latter substance does not appear to be so simple as that of the former; and I am at a loss to account for the differences in our results, unless they may be referred to differences in the food of the serpents. It is not, I presume, yet known, what changes may be effected in the excrementitious matters of animals, by such changes in their diet as may be compatible with their healthy existence.

Cork Institution, Oct. 18, 1819.

LII. Notices respecting New Books.

The Entomologist's useful Compendium; or, An Introduction to the Knowledge of British Insects; comprising the best Means of obtaining and preserving them, and a Description of the Apparatus generally used; together with the Genera of Linné, and the modern Method of arranging the Classes Crustacea, Myriapoda, Spiders, Mites and Insects, from their Affinities and Structure, according to the Views of Dr. Leach. Also an Explanation of the Terms used in Entomology, a Calendar of the Times of Appearance and usual Situations of nearly 3000 Species of British Insects; with Instructions for collecting and fitting up

* Philosophical Transactions 1818.

Objects for the Microscope. Illustrated with Twelve Plates.
By GEORGE SAMOUELLE, Associate of the Linnæan Society of
London. Svo. pp. 496.

WE have been much pleased with the inspection of this new and truly original contribution to the history of British Entomology. It bears manifest marks of being the fruits of much laborious and scientific research into a very interesting department of knowledge, and is presented to the public in a style of minute elegance and accuracy highly worthy of the interest of the subject which it illustrates.

Our readers will not be surprised at the terms in which we commence our notice of this work, when they learn that the author (Mr. Samouelle) has been indebted (see Dedication and Preface) "for the most valuable parts of its contents to the kindness and liberality" of Dr. Leach, F.R.S., who gave the author "the free use of his books and manuscripts." "It was an offer," says Mr. S. "which I could not withstand, and which no lover of science will regret." We are quite sure that no lover of science will regret it; every one must, on the contrary, feel inexpressibly gratified that the treasures of so able and indefatigable a cultivator of the science of Entomology as Dr. Leach, should be opened to general participation, and equally so that the task of unfolding them to the world should have devolved on an individual so faithful, ingenuous, and discriminating, as the author of the present volume has shown himself to be.

Mr. S. commences his work with giving a sufficiently ample and minute account of those characteristic parts of insects to which the attention of the student should be chiefly directed, and follows it up by some acute observations intended to show the superiority, we might almost venture to say the absolute necessity, of the modern System of Classification.

The object of all system is to reduce a science to its simplest terms, by reducing the propositions it comprehends to the greatest degree of generality of which they are susceptible. A good method in comparative anatomy must, therefore, be such as will enable us to assign to each class, and to each of its subdivisions, some qualities common to the greater part of the organs. This object is to be attained by two different means, which may serve to prove or verify one another. The first, and that to which all men will naturally have recourse, is to proceed from the observations of species to uniting them in genera, and to collecting them into a superior order, according as we find ourselves conducted to that classification by a view of the whole of their attributes. The second, and that which the greater part of modern naturalists have employed, is to fix beforehand upon certain bases of divisions,

agreeably to which, beings, when observed, are arranged in their proper places.

The first mode cannot mislead us; but it is applicable only to those beings of which we have a perfect knowledge: the second is more generally practised, but must be allowed to be subject to error. When the bases that have been adopted remain consistent with the combinations which observation discovers, and when the same foundations are again pointed out by the results deduced from observation, the two means are then in unison, and we may be certain that the method is good.

In systematizing the anatomy of animals, science is most deeply indebted to the learned, acute, and indefatigable Cuvier, who has contributed more than all others (save Hunter) to our accurate knowledge of the characters on which the classes are founded.

The whole animal kingdom is by Cuvier divided into four great types: *Vertebrosa*, *Mollusca*, *Annulata*, and *Radiata*. The animals which come under the observations of the author of the present work belong to the type *Annulosa*, which are divided into five classes: *Crustacea*, *Arachnöidea*, *Acari*, *Myriapoda*, and *Insecta*.

Mr. S. has under each of these heads given a review of the science as particularly connected with it, added to each genus its various synonyms, and particularly pointed out the species which serve as the types under the modern system. The characters on which the genera are founded, appear to us very simple and natural, and are rendered perfectly intelligible by the copious explanation which Mr. S. annexes of the terms used in Entomology.

The author gives towards the conclusion of his work an Entomologist's Calendar for each month of the year, pointing out the particular seasons and places where certain insects are to be found from January to December. Persons who reside at a distance from the metropolis must derive great advantage from this calendar, as, by carefully examining such places as are referred to in the calendar, they will not only meet with the species enumerated, but are likely to capture many new insects which have not yet found a place in the hitherto neglected Entomology of Britain.

The work is concluded by an explanation of the plates, which contain very beautifully delineated figures of between 200 and 300 insects.

We need scarcely add our opinion that, on the whole, the work is one which does much honour to the industry and science of Mr. Samouelle, and will repay in knowledge much more than it can possibly receive from the necessarily limited sphere of scientific patronage.

A portion

A portion of the following Entomological work is ready for publication :

Horæ Entomologicæ: or, Essays on the Annulose Animals: by W. S. MACLEAY, Esq. A.M. of Trinity College, Cambridge. Vol. I. Part I. containing general Observations on the Geography, Manners, and Natural Affinities of the Insects which compose the Genus *Scarabæus* of Linnæus; to which are added a few incidental Remarks on the Genera *Lucanus* and *Hister* of the same author.

A most entertaining work is now in the press, consisting of a valuable series of Anecdotes collected and arranged under separate heads, by Sholto and Reuben Percy, brothers of the Benedictine Monastery, Mont Bengier. The collection is, we understand, the fruit of much curious reading during many years of monastic seclusion; and, while it embraces a vast fund of entirely original matter, will omit nothing particularly worthy of preservation in the anecdotal treasures either of ancient or of modern times. The first four parts will consist of Anecdotes of Humanity, embellished with a portrait of William Wilberforcé, Esq. M.P.; Anecdotes of Eloquence, with a portrait of Lord Erskine; Anecdotes of Enterprise, with a portrait of the lamented Mungo Park; and Anecdotes of Youth, with a portrait of Robert Charles Dallas, son of Sir George Dallas. These will be followed by Anecdotes of Science, of Genius, of Liberty, of Heroism, &c.

De l'Industrie Française, par M. Le Comte CHAPTAL, *Membre de l'Institut.* 2 vols. 8vo.

[From the French of M. CH. DUPIN, in *Revue Encyclopédique*.

Count Chaptal, after being ten years engaged in the internal administration of France, in protecting the arts, sciences and letters, by creating for French industry establishments and institutions whose utility has saved them from subversion, now presents the world with the existing picture of that industry to the prosperity of which he has so highly contributed. The execution of such a task could have devolved on no person with more propriety.

The work is divided into four parts; which treat of commerce, of agricultural industry, of manufacturing industry, and of the administration of industry.

The part which partakes most of that sort of talent and information peculiar to M. Chaptal, is that which relates to manufacturing industry.

France has within the last forty years, by the aid of science, and impelled by the spur of necessity, made vast strides in this department. It now enjoys the fruits of the many and great

sacrifices which this conquest of knowledge and of industry has cost.

All the world knows the immense services rendered by the men of science in France, in extracting from the soil of France those means of defence which it required, when the other nations of Europe precipitated themselves with all their power upon her. The efforts which it was necessary to make for this purpose gave an impulse to the useful arts, which is still felt and will long continue to be felt.

Among the œconomical arts which have experienced the most remarkable extension, and which new or more improved processes have regenerated, are the manufactures of cotton thread and cotton cloth.

In 1789 the average value of the cotton goods imported into France, amounted to nearly 26,000,000 of francs; in 1812 it did not exceed a million and a half.

In 1812 a million of wheels were in activity, and spun 10,000,000 kilogrammes of cotton annually.

The art of manufacturing cloth is a branch of industry in which the French have always excelled.

Among their most beautiful fabrics the cachemires of M. Ternaux may be distinguished, as yielding not even to those of India, and as likely to become the first in the world, if the goats which this ingenious and enterprising speculator has imported should come to be naturalized in France.

At the head of the improvers of an important and difficult branch of art—that of watch-making—stands M. Breguet. — MM. Janvier, Pons, Lepaute Robin, follow in his steps. Fortin and Lenoir have carried to a high pitch of perfection the construction of philosophical instruments.

The chemical present results even more astonishing than the mechanical arts.

The chemical manufactories of France are the finest in Europe. M. Chaptal was the first to organize and perfect these scientific establishments. Every one knows how much the preparation of wines owes to his genius. The distillation of brandy and of spirits of all sorts has been also greatly improved by the united labours of MM. Chaptal, Argand, and especially Edward Adam.

The distillation of wood for the purpose of extracting vinegar, tar, &c. is an art of French invention, which dates its origin from the revolution.

The art of rendering waters salubrious by chemical filtration is also among the benefits rendered to humanity.

Metallurgy in all its branches has assumed a new aspect. Our iron-foundries have been improved, and our cutlery may now stand a competition with the best productions of England.

Our

Our porcelain and our pottery have likewise risen into high esteem. In this respect, as in many others, we have ceased to be tributary to England.

Lithography, which owes so much to the ingenious perseverance of M. de Lasteyrie, is daily improving; and must increase prodigiously the commerce in engravings, besides rendering numerous branches of instruction more æconomical.

In the manufacture of crystal we equal the English in quality, and surpass them in the elegance of our forms.

M. Chaptal presents afterwards a statistical view of our manufacturing industry in its present state: but it would take up too much room to analyse this chapter, which necessarily abounds in tables, and is therefore little susceptible of a concise yet sufficiently detailed abstract.

Statistique de la Suisse, &c.—Statistical Description of Switzerland, and the Twenty-two Cantons of which it is composed. By Professor PICOT, of Geneva. 1 vol. 12mo. pp. 574.

Professor Picot has rendered an important service to statistics and geography by the present publication.

It would give us much pleasure to see similar works on each of the other states of Europe, drawn up with equal care and with equal choice in the details. The collection which would be thus produced would be invaluable to statesmen, to travellers, and to readers of all classes. The cheap form in which this work appears is deserving of especial notice and praise.—Works of topography, which ought to be placed within the reach of every body, are generally with us brought out on so expensive a scale, as to be the least accessible of all modern publications. The work of M. Picot is deficient in one important particular. It wants a good map of Switzerland with the present divisions and subdivisions of the twenty-two cantons.

Revue Encyclopédique, ou Analyse Raisonnée des Productions les plus remarquables dans la Littérature, les Sciences, et les Arts. Paris, 1819.

This is a new monthly publication begun at Paris at the commencement of the present year. It has already attained a great reputation. The list of regular contributors to it comprehends some of the most eminent names in France: MM. Chaptal, Al. de la Borde, Dupin, David, De Lacépède, Langles, Lemercier, Magendie, Orfila, Sismondi, &c. It is scarcely necessary to add that it is distinguished for the value of its intelligence and the ability of its criticisms.

LIII. *Proceedings of Learned Societies.*

ACADEMY OF SCIENCES, PARIS.

ON the 16th of June the following New Observations on oxygenated Water, by M. Thenard, were read before the Academy.

In the observations on oxygenated water which I last had the honour of submitting to the Academy, I endeavoured to prove that water saturated with oxygen contains exactly twice as much oxygen as pure water; or, in other words, that pure water at a temperature of zero, and under a pressure of 0.76 metre, can absorb 616 times its volume of this gas. At the same time I stated the physical properties of this new liquid, and the phenomena produced by its contact with various mineral bodies. Since then I have examined its action on almost all mineral substances, and also on many vegetable and animal bodies. I will not now state all the results, but will mention one which seems worthy of attention; namely, that several animal substances are, like platinum, gold, silver, &c., capable of disengaging the oxygen from oxygenated water, without undergoing any alteration, at least when the liquid is diluted with distilled water.

I diluted pure oxygenated water in such a manner that it contained eight times its volume of oxygen. Of this I passed 22 measures into a tube filled with quicksilver; and then I introduced a little fibrin, quite white, and recently extracted from the blood. Instantly the oxygen began to separate from the water, and the quicksilver in the tube sunk. At the end of six minutes the water was completely de-oxygenated, and would no longer effervesce with oxide of silver. The gas disengaged was 176 measures; that is, as much as the liquid contained; and was entirely free from carbonic acid and azote. It was pure oxygen. When the same fibrin was placed in contact with fresh portions of oxygenated water, it acted in the same manner.

Oxygen is not disengaged from water, even much oxygenated, by urea, by liquid or solid albumen, or by gelatin; but the tissue of the lungs, in thin slices and well washed, that of the kidneys and of the spleen, expel the oxygen with as much facility as fibrin. The skin and veins have the same property, but in a less degree.

As the tissue of the lungs, the spleen, the kidney, like gold, platinum, silver, &c. possess the property of expelling the oxygen from oxygenated water, it is very probable that all these effects are attributable to the same force. Would it then be unreasonable to ascribe all animal and vegetable secretions to the same force? I think not. In this way we may conceive how an organ, without absorbing any thing—without giving off any thing—may

—may act constantly on a liquid, and transform it into new products. This way of viewing the subject agrees with some ideas lately suggested, and which, in some measure, become obvious from the experiments just recited.

LIV. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

October 11, 1819.

SIR,—As the Nautical Almanack is of the first importance to the safety of navigation and commerce, I trust you will be able to afford the following remarks a place in the Philosophical Magazine, as they are written purposely to call the attention of the Commissioners of Longitude, if possible, to adopt a *more certain method* of supplying the demand for that invaluable work, than hitherto experienced.

It is notorious that the Nautical Almanack has been frequently out of print for ten, fourteen, or twenty days, when a single copy could not be obtained for any price, nor until a new edition issued from the press:—commanders of ships, proceeding on distant voyages at such times, have been known to offer ten and even twenty guineas for a Nautical Almanack, without effect.

To prevent a similar recurrence of such serious importance to commerce and navigation, a letter was sent in 1811 to one of the Commissioners of Longitude, pointing out the great danger that would probably ensue to British maritime commerce by a limited supply of the Nautical Almanack; and recommending that the Secretary of the Board of Longitude, or some other competent person, ought to superintend the disposal of that work, by waiting on the publisher *monthly*, and examining the quantity sold, and stock on hand; which would enable such person to provide regularly for the demand, by comparing the monthly or quarterly sales of each year, and regulating the press accordingly; which ought either to be kept open, or larger editions thrown off than hitherto.

Since the above-mentioned letter was written, the Nautical Almanack has been several times out of print, and very recently the edition for 1820 was destroyed by fire at Mr. Bensley's, the printer to the Commissioners of Longitude; by which misfortune, an interval occurred, when ships departing on distant voyages could not procure a Nautical Almanack for that year. But fortunately a new edition appeared before the departure of Sir Thomas Hardy's squadron for the South Sea, or that valuable expedition might have been obliged to navigate nearly round the world, destitute of one of the first requisites of safety, a Nautical Almanack for the ensuing year.

A work

A work of such consequence, when printed, should not be placed under one roof, but sheet by sheet, as they are worked off, ought to be separated in two or three divisions, and placed in different buildings, in order to preclude a total loss by fire, and consequently not deprive the public of the temporary use of that valuable guide.

Four days ago I had occasion to inspect the Nautical Almanack for the present year, previous to observing the transit of the sun, in order to erect a sun-dial in my garden; but, to my surprise, I was told that the last edition of the Nautical Almanack for 1819 was all sold, and not a copy to be procured, nor would another edition be printed, as the year would soon end!—For the safety of lives and property, and for the honour of our country, I trust this is not the case, although I have made several applications for an Almanack, and received similar replies. Perhaps the publisher may consider it of little or no benefit to him, to print another edition of the Nautical Almanack so late in the year; but as there are two months and twenty-two days (or *eighty-three* days) of the year unexpired from the time that the last edition of the Almanack was sold off, and consequently out of print, it becomes a matter of serious importance and regret, that all the British shipping destined on foreign voyages, which sail between the 7th of October 1819 and the 1st of January 1820, must for the greater part grope their way by dead reckoning, (as in the infant state of navigation,) if they cannot procure the *Connoissance des Temps* from France, or some other equivalent from the Continent!—A remedy ought certainly to be provided by the Commissioners of Longitude for these irregularities in the publication of a work so necessary to the safety of navigation as the Nautical Almanack.

I am, sir, yours obediently,

AN OBSERVER.

SPODUMENE.—FLUOR SPAR.

We understand that spodumene was discovered last year by Dr. MacCulloch on the west coast of Scotland. It occurs embedded in granite, and may now be added to the list of British minerals, which has of late received such large accessions by his exertions, and by that of other mineralogists in this country.

We are also informed that he has discovered fluor spar in several parts of Aberdeenshire. It is almost always found in veins traversing granite; generally accompanied by quartz, and sometimes by galena. At Abergeldie it occurs crystallized in the most common form of cubes, and of various colours, blue-purple, crimson-purple, green, and white. The green varieties present a remarkable property, which has not yet called forth the attention of mineralogists; becoming white, or colourless, by

by exposure to air, and this effect penetrating to an inch or more in depth. It will be an interesting object for chemists versant in the analysis of minerals, to inquire respecting the nature of this colouring matter; a circumstance as yet very obscure in all the varieties of fluor spar. It presents an analogy to the topaz, of which the yellow colour is dissipated by a low heat without altering the transparency of the mineral; and, instead of arising from chemical composition, may possibly be of an optical nature; as by the loss of water, or other causes, the arrangement of the particles that determine the reflection of the green rays may be changed.

PRIMARY SANDSTONE.

We are indebted to Dr. MacCulloch for the first discovery of this important rock, which had been either unobserved, or confounded with the secondary red sandstone; the old red sandstone of the Wernerian system. The first account of it was contained in his papers on the Isle of Sky, published in the Geological Transactions, and, in these, its geological characters, and its claims to a place in the class of primary rocks, were satisfactorily proved. We understand that a fuller account of it will be contained in his work on the Western Islands of Scotland, now on the eve of publication; where he has shown that it occupies an extensive district on the western coast. We are sorry, however, to see that the map which he has given, in which that rock appears, (of which we have seen the proofs in the publisher's hands,) does not contain the whole of that coast, as well as the islands; having been curtailed to accommodate the size of the volume. We wait with impatience for these details, as the discovery of a sandstone and a conglomerate alternating with gneiss, must form an important revolution both in the theories of our geologists, and in the science of geology.

QUARTZ ROCK.

It is to the same industrious geologist that we are indebted for having first ascertained the true characters of this rock in all its varieties, and the several connexions under which it appears among the primary strata, to which it belongs. His predecessors were at a loss what to do with it in their systems; nor had they observed the extent of its range, or the nature and variety of its connexions. His papers on this subject were printed in the Geological Transactions, apparently, just as the several facts had occurred to him in succession. As he has there described it forming the island of Jura, we expect to find a fuller detail of it in the work above mentioned, where that island is described.

It appears, from those accounts which we have read, to occupy a considerable space in Scotland; occurring in alternation
with

with gneiss, primary sandstone, micaceous schist, and, if we do not misapprehend the statements, clay slate.

METHOD OF RENDERING GLASS LESS BRITTLE.

Let the glass vessel be put into a vessel of cold water, and let this water be heated boiling hot, and then allowed to cool slowly of itself, without taking out the glass. Glasses treated in this way may, while cold, be suddenly filled with boiling hot water without any risk of their cracking. The gentleman who communicates the method, says that he has often cooled such glasses to the temperature of 10°, and poured boiling water into them without experiencing any inconvenience from the suddenness of the change. If the glasses are to be exposed to a higher temperature than that of boiling water, boil them in oil.—*Annales de Chim. et de Phys.* ix.

EARTHQUAKES.

Three dreadful earthquakes took place at Copiapo on the 3d, 4th, and 11th of April. The whole city is said to have been destroyed by these awful visitations. More than three thousand persons were traversing the neighbouring plains, flying from the desolation which had been produced. It appears, according to all the accounts, that the inhabitants had time to save their lives, but only their lives. Copiapo is a sea port of Chili, and stands on the south side of a river of the same name, about 490 miles N. by E. of Valparaiso.

Another severe shock of an earthquake was felt in Trinidad on the 12th of August at half past 2 A. M. A rushing noise as of a violent wind was first heard, which was instantly succeeded by an undulatory motion from east to west, very severe, and which lasted four or five seconds. It was a clear moonlight night, and nothing particular was discernible in the state of the atmosphere.

On the 15th of August, a shock, accompanied with an explosion as loud as that of a cannon, was felt at the village of St. Andrews, in Lower Canada.

EXTRAORDINARY VISITATION.

A letter from Green Bay, Mich. Territory, United States, dated July 19, 1819, gives the following very curious account of the visitation of that country by clouds of insects, which will bear a comparison with the swarms which heretofore darkened the air of Egypt in their flight:

“Within the last four or five days *the fly* has appeared—a non-descript perhaps in natural history—and covered the face of the whole earth, obscuring the sun, moon, and stars. I write literally, and without the least exaggeration. The heavens are darkened

darkened by them, as in a densely cloudy day; as far as the eye can discern, they fill the air; in every direction, as closely as a thick swarm of bees. Cornfields, &c. are prostrated with the clouds that settle upon them; trees are covered, and the branches bent and broken down. The barracks and buildings in the vicinity, at the ends and sides not exposed to the sun, are entirely black, the insects piled one upon another. These creatures, with their feelers that protrude from head and tail, are about three inches in length, slough their skins daily, it is said by the inhabitants here; and in performing this operation, and in dying by millions every hour, infect the atmosphere so that it becomes unfit to breathe. Cattle, swine, and Indians, are said to feed and fatten upon them. The Frenchmen call them musquito hawks, because they make their appearance when mosquitoes are most numerous, and, as is supposed, prey upon and drive them away. 'The flies themselves remain but six or seven days.'

POISONOUS CONFECTIONARY.

To Mr. Tillock.

Hackney, Oct. 10, 1819.

SIR,—I have observed in the Philosophical Magazine for September an article on poisonous Tea Leaves, which calls to my mind a highly blameable practice of contaminating sugar drops with a substance very injurious to health.—Some time ago, while residing in the house of a confectioner, I noticed the colouring of the green fancy sweetmeats being done by dissolving sap-green in brandy. Now sap-green itself, as prepared from the juice of the buckthorn berries, is no doubt a harmless substance; but the manufacturers of this colour have for many years past produced various tints, some extremely bright, which there can be no doubt are effected by adding preparations of copper.

The sweetmeats which accompany these lines you will find have evident vestiges of being contaminated with copper.—The practice should therefore be banished of colouring these articles of confectionary, the proprietors of which are not aware of the deleterious quality of the substances employed by them.

I am yours respectfully,

GEORGE MILES.

HERCULANEUM MANUSCRIPTS.

Rome, Aug. 10.

A third volume of the MSS. of Herculaneum is in the press, and will soon be published. Sir Humphry Davy is expected in September to make experiments with the chemical composition which he has invented to unrol the ancient Latin MSS. of this collection. It has been observed that the Latin MSS. in papyrus are

are covered with a peculiar varnish which increases the difficulty of unrolling them, and which the Greek MSS. have not.

GRECIAN UNIVERSITY.

A university has been established at Corfu, by Lord Guildford, under the direction and auspices of the British Government. His Lordship has appointed to the different chairs, Greeks of the first abilities; and his intentions have been seconded with much effect by Count Capo-d'Istria, a native of Corfu, who being apprized that M. Politi, a young Leucadian possessed of knowledge and talents, desired to profess chemistry in the Ionian islands, remitted to him funds sufficient to procure the apparatus necessary for the laboratory, &c.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Thompson, manufacturer of iron and coal miner, for a new method of extracting iron from ore.—20th Sept. 1819.

To Baron Charles Philip de Thierry, of Bath-Hampton in the county of Somerset, for a bitt for coach and bridle, called The humane safety-bitt.—20th Sept.

To John Baynes, of Leeds, cutler, for certain machinery to be attached to carriages for giving them motion by manual labour, or other suitable power.—27th Sept.

To William Bainbridge, of Holborn, musician, for certain improvements in the double and single flageolet or English flute.—4th Oct.

To Jacob Perkins, late of Philadelphia, now of Austin Friars, engineer, for certain machinery and improvements applicable to ornamental turning and engraving, and to the transferring of engraved or other work from the surface of one piece of metal to another piece of metal, and to the forming of metallic dies and matrices, and also improvements in the construction and method for using plates and presses for printing bank notes and other papers, whereby the producing and combining various species of work is effected upon the same plates and surfaces, the difficulty of imitation increased, and the process of printing facilitated; and also an improved method of making and using dies and presses for coining money, stamping medals, and other useful purposes.—11th Oct.

To Christopher Hilton, of Darwen, near Blackburn, Lancashire, bleacher, for his process for the purpose of improving and finishing manufactured piece goods.—18th Oct.

To Anthony Radford Strutt, of Makeney, Derbyshire, cotton spinner, for certain improvements in the construction of locks and latches.—18th Oct.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1819.	Age of the Moon.	Thermo- meter.	Baro- meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
Sept. 15	26	74.	29.50	Fine—heavy rain in the evening
16	27	52.5	29.60	Cloudy—ditto this morning
17	28	60.	29.83	Ditto
18	29	65.	29.83	Fine—brisk wind
19	new	58.	29.93	Ditto ditto
20	1	59.	30.29	Ditto
21	2	60.5	30.23	Cloudy
22	3	60.	30.23	Fine
23	4	63.	29.96	Ditto
24	5	62.	29.56	Ditto
25	6	67.	29.26	Ditto
26	7	59.	29.30	Ditto—rain in the afternoon
27	8	64.	29.30	Cloudy
28	9	66.	29.25	Fine
29	10	57.	29.30	Rain
30	11	68.5	29.33	Cloudy
Oct. 1	12	71.	29.33	Ditto—brisk wind
2	13	70.	29.40	Fine
3	full	66.	29.30	Ditto
4	15	57.	29.26	Cloudy
5	16	49.5	29.86	Fine
6	17	55.	29.70	Cloudy
7	18	58.	29.57	Rain
8	19	64.	29.76	Cloudy
9	20	63.5	29.58	Fine—rain P.M.
10	21	70.	29.50	Ditto
11	22	70.5	29.40	Ditto
12	23	64.	29.70	Cloudy
13	24	67.5	29.60	Ditto
14	25	62.	29.80	Ditto—rain P.M.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For October 1819.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock Morning.	Noon.	11 o'Clock Night.			
Sept. 26	56	62	56	29.65	30	Showery
27	57	65	57	.74	0	Rain
28	62	60	60	.73	0	Rain
29	60	64	58	.72	0	Rain
30	68	66	62	.90	36	Showery
Oct. 1	66	70	62	.85	46	Fair
2	66	69	59	.83	46	Fair
3	65	68	56	.76	22	Showery
4	60	59	47	.66	34	Cloudy
5	42	53	43	30.08	52	Fair
6	45	55	53	.04	28	Cloudy
7	55	62	57	29.96	29	Cloudy
8	60	67	60	30.13	36	Cloudy
9	60	66	60	29.90	34	Fair
10	63	72	63	.90	36	Fair
11	64	72	62	.96	48	Fair
12	62	78	58	30.08	42	Fair
13	61	68	53	.05	36	Fair
14	56	63	53	.19	30	Cloudy
15	53	62	51	.39	29	Fair
16	51	55	46	.30	26	Cloudy
17	43	51	41	.24	38	Fair
18	42	51	43	.30	39	Fair
19	38	53	53	.10	34	Cloudy
20	55	54	46	29.64	0	Rain
21	38	37	37	.61	0	Snow showers and a heavy fall of snow
22	32	45	40	.50	18	Fair [in the night
23	37	51	41	.36	19	Cloudy
24	40	47	40	.42	19	Cloudy
25	36	43	38	.55	24	Fair
26	40	47	36	.73	29	Fair

N.B. The Barometer's height is taken at one o'clock.

LV. *Some further Remarks on Swallows.* By Mr. GAVIN
INGLIS.

To Mr. Tillock.

DEAR SIR, **H**AVING already intruded so much on the patience of your readers with my remarks on the sagacity and habits of the swallow, I feel rather inclined to apologize for troubling you with any thing further regarding these wonderfully wise little creatures. Still, however, I must encroach, to state a few circumstances which may amuse some of your friends, being corroborative of what was formerly stated. I shall, however, endeavour to be as brief as may be consistent with perspicuity.

In my former communications I stated minutely the circumstances attending the departure of the White Martin, or what is known in this part of the country by the name of the White-tailed Swallow, at an unusually early period of last year. These returned this year as formerly, the same morning, and in company with the Chimney Swallow, on the 15th of April, and brought forth two successive incubations. The first was on wing by the end of June, and the second early in September. About the middle of that month the whole Martin tribe disappeared; but the Chimney Swallows remained in considerable numbers till the 3d instant, when they also took their departure.

The hibernal departure of this colony of Chimney Swallows may this year have been protracted in consequence of a few tiles having been blown from the roof of the boiling-house in a gale of wind, by which means some of the bleach-field cats had on a Sunday, when no person was near to protect the swallows, made their way through these openings, and reached two nests when the young brood was in a pretty advanced state. These were immediately devoured by ravenous puss. The tiles were replaced on the Monday morning; on which the old swallows resumed possession of the nests, and in due time produced another hatching of young birds.

From the time lost by the destruction of the former, this hatching was considerably later than the collateral young of the second incubation, and seemed, after their kindred brood from the other nests were on wing, and able to provide for themselves, to become the adopted children of the colony. The feeding, training, and protecting them alike devolved on all, and became one common concern.

After these nestlings were able to roost on the house-top, I have frequently seen, toward evening, dozens of the old swallows al-

ternately feeding these young ones till they were completely gorged, and then returning to rest for the night.

The circumstance of the cat's devouring the intermediate hatching rendered the old swallows doubly suspicious of the meanderings and movements of tabby.

One morning, soon after this catastrophe, I had gone out of doors to examine the state of my ley cisterns, and was attentively observing a curious motion in the alkaline lixivium that had been left at rest over-night. I was soon induced to relinquish my musing, by the noisy shrieks of a group of swallows that were flying round and hovering about me, and directing not their attacks, but their movements, in such a manner as seemed to claim my attention. I had no sooner raised my eyes from looking at the liquor in the cisterns, and turned myself towards them, than they seemed satisfied I had listened to their complaint; they then made a circuit round me, and in a body darted into an open house with great velocity and noise. Finding I did not follow them, they immediately returned and flew round and round me so close as to make me feel the wind from their wings, and again darted into the house, returning instantly with augmented celerity and clamour, repeating the same movements and indications of distress, quickening their motions as the danger had become more apparent. It then struck me that the extreme distress of the swallows must proceed from some cause of alarm within the house, and that by these movements they were using all their art to decoy me into that place, and anxiously importuning my protection against some common foe. On conceiving this, I immediately followed them, and was fully confirmed in my conjecture, on finding a cat perched upon a plank that had been incautiously left, *as if* to enable some of the tribe to reach a nest, and in the very act of placing herself in a position to take the leap. Puss, on seeing that the swallows had procured this timely assistance, betook herself to flight; when the swallows ceased their clamour, perched, and began trimming their feathers while I stood by them, as if nothing offensive or alarming had occurred.

A very striking instance of their discriminating discernment occurred about this time, which showed their courage in attacking their foes, their disposition to resent an injury, their determination to inflict punishment on the aggressor, and that neither time nor change of circumstances could divert them from their purpose of keeping up a continued warfare against the offender, till the day of their final departure for the season. One of the men-servants, standing on a steeping vessel, reached up his hand and took a young swallow from a nest immediately above him, in presence of one of the parent swallows; who, being previously acquainted

quainted with the individual, showed no symptoms of uneasiness at what passed, till he, to try if the young one could fly, threw it a little up from his hand. The young bird spread its wings, but, being insufficiently fledged to support itself, fell to the ground, and was killed in sight of its afflicted dam, who immediately shrieked the war alarm: the same call was instantly repeated, echoed, and re-echoed from without, and crowds of swallows came flocking from all directions into the boiling-house. Upon ascertaining the state of matters, this man was singled out, and the marked displeasure of the whole group declared by their at once commencing open hostilities against him by all their varied modes of annoyance and attack, without deigning to take the least notice of any other individual present.

This continued day after day, at every time and in every place they could meet him, attacking him whenever he appeared out of doors, following him into the house, and darting at him in the midst of his companions, without the dread of being repelled. One instance of this particularly marked their thorough knowledge of the individual, and determination to be revenged: He had gone to a neighbouring farm in company with another man (also belonging to the bleach-field) to examine some flax, and on their way had to pass a plantation at the distance of nearly half a mile from the original scene of action, where a considerable number of swallows were busily catching flies under cover of the trees. The two men had no sooner turned a corner, and come in view of the feathered tribe, than one of them singled out the object of their dire resentment by darting full in his face, and at the same time shrieked the battle call. The group left off fly-catching, and gathered in an instant round the men, never all the while taking the least notice of the guiltless individual, but directing all their vengeance against the transgressor. In the most determined and resolute manner they kept up their annoyance the whole breadth of a large grass field, vexing him to such a pitch that, to relieve himself of these troublesome attendants, on coming to some fallow land, he was obliged to collect clods and gravel and throw amongst them with all his force:—upon this they retreated, and betook themselves to the wood. There they remained under close cover till his return, allowing him to pass nearly half the breadth of the grass field, where there were neither clods, stones, nor gravel to repel them, and commenced their attacks anew, with increased vigour and impetuosity, to the great amusement of his companion, till he reached the bleach-field, where he was glad to take shelter in one of the houses, declaring “*Tho’ thae Swallas sid big aboot the field thaes thousand year, bae my certy I’s nae mair fyle my fingers wi them.—Hech!!! sic a steur as I hae hain wi the bratts! wha wad a dree’d thaer kennin a body sac far afield?*” The

case of Mr. Thomas Diamond, of Brenchley, as stated in the Kent Herald, also strongly marks their faculty of discrimination, recollection, and disposition to retaliate.

Yours truly,

Oct. 9, 1819.

GAVIN INGLIS.

P. S.—This morning the water from the lake shows something like an internal commotion, although darker in the colour than on former occasions. Upwards of 3000 eels were taken last night in their way from the lake to the sea.—12th Oct.

LVI. *An Essay on Dreaming, including Conjectures on the proximate Cause of Sleep.* By ANDREW CARMICHAEL, M.R.I.A.

[Concluded from p. 264.]

MANY inquirers have been perplexed to account for the lively conversations we hold in our sleep, involving rational replies, sarcastic retorts, and alternating arguments. This, however, can be explained without any recurrence to the plurality of organs. Whatever we are capable of thinking without an effort, we are susceptible of dreaming; and during our *waking reflections*, we frequently imagine what kind of reply an adversary might make to an observation we had dropped—we immediately enter into the warmth of argument, by coining an answer of our own in return; and when we have said all that occurs on that side of the question, a reply naturally suggests itself on the other, all the merit of which we ascribe to our antagonist; and thus the disputation goes on, as if *two different minds* were engaged in the contest, the words by a strange illusion tingling in our ears, and the ardent looks and forcible gestures flitting before our eyes, till some real object, breaking on our attention, recalls us to the perception of the external world; and the nature of the reverie, which, till now, we thought real. In sleep there is no such intrusion; but the dream and the reverie do not differ from each other as long as they last.

A dream must, therefore, be the necessary consequence of any portion of the brain being awake, while the senses are asleep; and the question naturally occurs—Are the senses ever awake while the brain is asleep? and if so, what is the consequence? Certainly, not the perception of external things, because the sensorium being involved with the remainder of the brain in sleep, the rays of light would merely fall upon the retina, and the vibrations of the air on the auditory apparatus, without conveying any further the impressions of colour or sound. But if those sensories, and other
limited

limited portions of the brain, were awake with the organs of sense, the obvious and natural consequence is actually one of very common occurrence. The active organs continue to think; but their thoughts do not appear to be dreams, because our communication with the external world, by means of our senses, prevents this phenomenon; but at the same time we are sensible that we *are*, to use a common expression, half awake and half asleep; and there are few individuals who have not frequently indulged themselves in the luxury of observing the gradual departure of their slumbers, and the renewal of their active and proper existence. Yet, possibly, if the organ of a single sense is awake,—as for example, that of hearing or feeling,—its effects may not be altogether complete, but so far merely as to satisfy us of the illusory nature of our internal perceptions; and thus we are enabled to account for the extraordinary circumstance of *dreaming* that we are *dreaming*.

These several predicaments, therefore, present no less than seven different states of sleeping and waking:—1. When the *entire brain and nervous system* are buried in sleep; and then there is a total exemption from dreaming.

2. When *some of the mental organs* are awake, and *all the senses* are asleep; then dreams occur, and seem to be realities. If among these busy organs should be one or two, whose peculiar powers and affections will readily occur to the admirers of the organic theory, their disturbance, whether occasioned by disorder of the digestive functions, or any other cause, will sufficiently account for uneasy dreams, frightful visions, and oppressive night-mares.

3. When the above-mentioned conditions exist, and the *nerves of voluntary motion* are also in a state of wakefulness, then may occur the rare phenomenon of somnambulism.

4. When *one of the senses* is awake with *some of the mental organs*, then we may be conscious during our dream of its illusory nature; and if the *nerves of voluntary motion* concur, somnambulism may also accompany these circumstances.

5. When *some of the mental organs* are asleep and *two or more senses* awake, then we can attend to external impressions, and notice the gradual departure of our slumbers; a state in which we consider ourselves neither awake nor asleep.

6. When we are *totally awake* and in the full enjoyment of all our faculties and powers.

7. When under these circumstances we are so intimately occupied by our mental operations, as that we *do not attend to the impressions of external objects*; and then our reverie deludes us like a dream.

Thus, as the process of assimilation ceases to operate, one por-

tion after another of the brain and nervous system is restored to a state of vigilance and energy: and thus the verification of this conjecture, in conjunction with the explanations afforded by the organic theory, will be fully adequate to remove all that has been obscure and inexplicable in these mysterious phenomena.

These views may also furnish a hint to physicians on the probable causes of one or two common affections of the head. If the process of assimilation is continued in the brain after a due interval of rest, and that a portion of this viscus is asleep, while the remainder and the organs of the senses are awake, the concomitant stupor and dulness may well be identified with the most frequent species of head-ache, which scarcely amounts to pain, and is little more than a lethargic and sluggish inertness accompanied by mental confusion and ineptitude: but if, on the contrary, the assimilating process is defective, and that the substance of the brain is not sufficiently renewed, a different species of head-ache may be the result; but it is always attributed to a concurrent effect—the absence of sleep. It is, however, obvious that neither this nor the former affection can be confounded with that acuteness of pain which is connected with the over-distention and pressure of the vessels on the brain.

It is also to be inquired, whether more serious disorders may not be the consequence of disturbance or partial suspension of the process in question. How often is mania preceded by protracted watchfulness? and is not a full allowance of nourishment considered, by the most respectable modern practitioners, as one of the most indispensable requisites in the treatment of insanity? and is not the return of intellect in general preceded by the restorative action of sleep? It may indeed be maintained by an advocate of the organic theory, that madness is sufficiently accounted for by the protracted over-excitement of a particular organ; but this very over-excitement, according to the hypothesis, causes the absorption and waste of some portion of the brain; and its protracted duration may interrupt the healthy action of that very process, which alone can renew the exhausted substance whose instrumentality is necessary to the operations of the mind. If the healthy action of this process can be restored, it is accompanied by refreshing reinvigorating sleep; and in numerous cases, if the natural consequence is the returning health and sanity of the patient, in others we may be disappointed, should the malady have proceeded so far as to injure or destroy the organization on which the intellect depends. It is by no means, however, contended that the cause assigned will account for every species of mental derangement; and it may, in none of them, be more than a concomitant of some more operative, but unknown cause.

If this theory will account for some disorders, it may also be serviceable in indicating remedies, and it may possibly instruct us that in many cases, and perhaps in most, the best opiate that can be administered is natural and comfortable nutriment, which, by instigating the process of assimilation, may bring on the most profound and healthful slumbers. In some cases, a great loss of blood is followed by restlessness and total deprivation of sleep, accompanied by delirium. It is evident that these symptoms originate in the want of a sufficient quantity of blood to deposit the nutritive particles where most required in the frame, and particularly in the encephalon. To lessen that quantity, with a view of diminishing the apparent feverishness, would, under such circumstances, be death to the patient; but the opposite system of administering the comforts of wine and animal juices, and thus contributing to the increase of the blood, must have the most beneficial operation, by inducing the assimilating process, the consequent sleep, and all the cordial and concurrent effects of their powerful agency.

I should not have hazarded these latter observations, so little within my province, but that they were suggested by facts harmonizing with and supporting the hypothesis of sleep which I have ventured to propose—and if that hypothesis shall ever be verified, the application I have made of these facts cannot fail to be useful. I am therefore unwilling that these views should be lost, and I am not so confident of their importance as to imagine they will speedily occur to another. With respect to the theory of dreaming advanced by Doctors Gall and Spurzheim, it is but one instance out of numberless others, in which their system will be found on investigation to correspond at every point with nature and truth; and if we are satisfied that they are right even in this one instance, we shall not be eager to reject the remainder of their singular doctrines, without affording them at least the advantage of an equitable, candid, and dispassionate examination.

The preceding arguments have been by some thought inconclusive, because no proof has been advanced, that during dreams the brain is partly asleep and partly awake. It is true this has not been *directly* attempted, for in the nature of things such an enterprise could not prove very successful. All that I considered requisite in the question, I trust I have accomplished, in showing the inadequacy and imperfection of the several theories which were most highly approved of, before the promulgation of Doctors Gall and Spurzheim's opinions; and that these philosophers have succeeded where their predecessors had failed, in explaining with precision and clearness all that was difficult or obscure in these perplexing phenomena. It was scarcely to be expected that I

should introduce, by way of episode, into an essay like this, the volume of facts and observations by which they have endeavoured to establish the plurality of organs. It is enough if my reader is satisfied that this fact, or let us be contented to call it this supposition, in accounting for every circumstance, affords a sufficient foundation for a rational and satisfactory theory of dreaming.

It has also been asserted that there are no grounds to suppose that the assimilating process does *not* occur in the brain except during sleep. Neither, it may be replied, is there any evidence of the affirmative—so far both sides of the argument are equally balanced; but it seems rather more probable that this process is in operation, not only in the brain but in every part of the frame at intervals of inactivity and repose. The athletic arm accustomed to laborious exertion, becomes every day more muscular and powerful; but if it were allowed no intermission from toil—nay, if it did not enjoy a due portion of quiet, does any one doubt but its vigour would diminish and its bulk waste away? It cannot, therefore, be the labour that increases its size—its influence can only extend to render it more fit to receive the deposition of the nutritive particles; and if that deposition be supposed to take effect on the cessation of exercise only, every circumstance included in these phænomena finds a distinct and easy solution.

What exercise is to the limbs, thinking is to the brain; and the latter, like the former, may be exerted to intensity, or relaxed to lassitude and listlessness. As motion, whether slothful or vigorous, so thought, whether feeble or powerful, seems an indispensable condition of being awake; it is caught from organ to organ, as this or that association stirs up their energy: but although only one be active at a time, the unity of the man and the concord of his volitions, appear to require that every faculty he possesses should exert an attention subservient to that peculiar power of the mind which happens to domineer for the moment; this passive vigilance and active intellection would, therefore, take their turn till the majority of the organs, in a state of fatigue or exhaustion, forgo all employment, and receive, *at rest*, a renewal of their substance from the process of assimilation.

But it may still be objected, that the nerves of organic life are always in action.—How therefore can they be renewed, if the process in question only takes place in an interval of rest? This is indeed a formidable difficulty, though perhaps not an invincible one, notwithstanding the obscurity of the subject. Of all the organs of the human frame, the heart is the most incessant in its motions; we may therefore confine our inquiries to the phænomena it displays. It has its systole and diastole, its contraction and expansion—during the former the nerves may be considered in a state of exertion, and in the latter in a state of relaxation

laxation and rest ; neither lasts long ; the alteration is most rapid :—yet if the designs of Providence require that assimilation should in general take place during an interval of rest, the present instance affords no exception ; and we may as readily conceive that the coronary vessels may repeatedly pour out the nervous secretion, at the moment the nerves remit their action, as that the condensing valve of the steam-engine should permit the escape of a due portion of water, at the requisite juncture. Thus the phenomena of organic life will present no anomaly in the arrangements of nature ; and the law which governs these circumstances, will be found not less general than any other of the laws established by the Creator.

It has next been objected, that the effects of the process of assimilation on the brain and nervous system, cannot be considered as the cause of sleep, inasmuch as sleep is incident to plants and many animals which are altogether destitute of brain and nerves. With respect to plants, the phenomenon is manifested by the drooping or folding together of their leaves or leaflets ; and this change is said to be occasioned by *the withdrawing of the stimulus of light*, and is merely *presumed* to be a state of rest to their vital functions*. But this circumstance, whatever may be its nature, is not by any means so general in the vegetable kingdom, as sleep in the animal ; and therefore its proximate cause cannot possibly be regarded as so indispensable an agent as the assimilating process, or whatever else is the proximate cause of the latter :—but the vital functions of plants have so little resemblance to the vital functions of animals, and are so utterly dissimilar to those superior functions which depend on a brain and nervous system, (which are exclusively concerned in the hypothesis,) that no accurate analogy can well be instituted between them—much less between the conjectural rest of the one, and the unequivocal repose of the other ;—and the naturalist, who would seriously attempt to establish the comparison, might as rationally pretend to ascribe both effects to one and the same cause, and decide that the sleep of animals, as well as plants, is occasioned by “ the withdrawing of the stimulus of light.”

But with respect to those inferior creatures which are destitute of brain and nerves, it is maintained by physiologists, that the nervous substance is irregularly diffused through the entire of their structure : there is therefore no necessity to seek for a peculiar explanation of their sleep. Whatever be their mode of imbibing nourishment, they must necessarily be in a state of wakefulness while employed in the act ; but after they have digested their food, it must, as in superior animals, be conveyed through their system, however little analogous the instruments of

* See Rees's *Cyclopædia*, article SLEEP OF PLANTS.

transmission, and deposited, as in them, in every part of their mass. This deposition taking place on the nervous substance intermingled with their texture, may be accompanied with the same result as when it affects more perfect systems; the difference between their sleeping and waking, it is true, is not so obvious, as where a larger portion of intelligence is suspended by the paralyzing effects of the process; and indeed the whole of their existence seems little better than a perpetual sleep.

A fourth objection insists, that if the process of assimilation be supposed the cause of sleep in hibernating animals, all their superfluous store of fat, converted into nervous matter, and deposited in their head, would swell their brain to so unconscionable a size, as to render the theory altogether incredible; and also, that the perspiration of those creatures sufficiently accounts for their meagerness at the end of their retirement, without supposing their previous obesity to be exhausted in the manner presumed in the hypothesis. In these objections, the action of the absorbents has been entirely overlooked. There is no reason to suppose that they discontinue their office; and if not, it is natural to think that they will scarcely suffer the brain to increase to any very unusual dimensions. And with regard to the phænomenon of perspiration, it will hardly be maintained that the fat will exude like oil through the pores of the skin, before it has been taken up in the usual way by the absorbents, and conveyed by them into the blood-vessels, and by *their* extremities deposited somewhere within or without the body. If within the body, the deposition of nutritious particles, being general, must, as well as elsewhere, take place upon the brain itself, in support of the litigated hypothesis; where having performed their duty for a time, they may be carried away as before, and detruded from the cuticular pores in the form of perspiration, though so lately employed in the ministry of the intellect, and perhaps not altogether inactively, if these animals dream.

The only remaining objection which has been advanced, notices the common occurrence of our dreaming on the subject which has most occupied our thoughts during the day—and contends, that if the peculiar organ has been exhausted by this exercise, it must be in a condition favourable to its renewal by the process of assimilation; and this process, wherever it is active, precludes the possibility of dreaming. The truth of this reasoning must be admitted; without admitting, however, as a fact, that we dream more frequently on the subjects which have occupied us during the day, than on other subjects: but when this circumstance occasionally occurs, is it inconceivable that the organ should receive a portion of refreshment during the night, and, from the very urgency and importance of the thoughts which occupied it during the

the day, return to them again in resistance and interruption of the process in question? Is it even impossible that the subject of reflection may have occupied it, without an interval, from the moment of retiring to rest, even though all the other organs may have sunk into repose? May not this incessant activity continue, for several days, to the detriment of the health? may it not even involve the entire brain, and, by preventing the accession of sleep, terminate at last in mental derangement?

Thus, however weighty and formidable these objections appeared, instead of subverting, they all contribute their aid to support the hypothesis.

But is it in the contemplation of those who dissent from these opinions to maintain that sleep is nothing more than repose after fatigue—that there is no other difference between waking and sleeping, than between labouring and abstaining from labour; and that there is no important vital process operating on the instruments of sensation, voluntary motion, and intelligence? If they allow that there is some *such* process, I should be glad they would point it out; and if it better accounts for these various phenomena than *that* of assimilation, I shall willingly relinquish my hypothesis in favour of theirs.

Will it be contended that the whole brain and nervous system may sleep during dreams? Then why are not dreams the constant attendants on sleep? Why are not all our visions accompanied by night-mare? and why is somnambulism so rare an occurrence? Will it, on the other hand, be averred that the whole of the brain may be awake while the residue of the man is asleep? Let this be admitted—but, if so, why are not the nerves of the senses and voluntary motion, which bear so strong an analogy to the brain, in substance and office, equally wakeful? Why are we not always in communication with the external world; or, in other words, Why are we not always awake? The process of assimilation relieves us from these difficulties, and shall we still be inclined to reject it? Perhaps, on these considerations, my hypothesis may happen to find favour, and that of Gall and Spurzheim be discountenanced:—perhaps it will be asserted that the assimilating process may lock up in sleep those parts of the brain allotted to reflection, judgement, and will, but leave to the active enjoyment of its inmates the local habitation of the imagination and fancy. Even this would be a more plausible conjecture, than the incongruous theory which insists that at the same moment the soul can be partially awake and asleep; but there is as little foundation, in *nature*, for the one as the other. Gall sought with indefatigable perseverance and adequate sagacity, for some external indication of the seats of those faculties, but none could be found; yet his labour did not go unrewarded. It led him to more fortunate

tunate observations; and, by the same rational process of inquiry which guided his illustrious contemporaries Herschel and Davy to such brilliant and stable discoveries, he and his celebrated co-adjutor have established in the brain the *probable*—I will not say, the *certain*, existence, until other philosophers have verified their results, as other philosophers have verified those of Davy and Herschel—they have established, I repeat, the probable existence of upwards of thirty material organs of the mind, each of which is endowed with its own peculiar desire, memory, and imagination. Admitting, therefore, the mere hypothetical existence of such organs, we try their verity by the indisputable test of some well-known phænomena; for instance, the phænomena of dreaming: and if it appears from incontrovertible reasons, that the various circumstances attendant on our dreams are utterly inexplicable on any other principles current at present than those of the organic theory, those principles have the *strongest* claims to be regarded as true. These two kindred hypotheses of dreaming and sleep afford each other a reciprocal support; and I shall be but too much honoured, if the philosophic authors of the *one* do not consider the *other* unworthy of such an alliance.

I flattered myself that I had answered every argument by which my essay could be assailed; but I understand that another objection has been advanced against it, which ought earlier or never to have made its appearance—the charge of materialism. It is necessary to ascertain the meaning of this word, which has so often, and for the most disingenuous purposes, been used without any. It has a threefold signification, and in two of its senses it conveys a manifest imputation of culpable absurdity or perverseness, fatal to virtue and subversive of society. In its third sense, whether it be guilty or innocent, I am not called upon here to discuss; for in none of its meanings, and I shall examine them all, is it imputable to my essay.

In its first and most reprehensible sense, materialism infers that the universe created itself—that the creation is without a Creator—that the mighty fabrick, evincing design in all that is minute as well as all that is magnificent, rose into being undesigned by infinite intelligence, unbidden by infinite power. It is scarcely possible to conceive, that opinions such as these can find an asylum in any rational mind;—even the last remnant of reason that sticks to a maniac would intuitively reject them. It will not be said that my essay countenances this doctrine, when every line of it breathes with an effort to discover and display the hidden arrangements of God, in what is to us the most curious of his works, the mechanism of man; and in the noblest part of that mechanism, the instruments of the mind.

In its second sense, materialism implies the double proposition that the human frame is untenanted by a soul, and that there is no future state of rewards and punishments. This opinion, as pernicious to society as the former, is not so much as glanced at in the essay. But if it be inferred, as a necessary deduction from the theory of Gall and Spurzheim (whose sentiments, so far as they embrace distinct and numerous organs of the mind, it is my pride to avow, and my ambition to vindicate), the inference could only be made by individuals who have not the good fortune to be intimately acquainted with their principles. The opinion which these philosophers universally maintain is, that the brain is the instrument by which the soul performs her intellectual operations; and I fancy that few will be hardy enough to maintain that the soul, in this life, ever performs those operations without one. Is there a physiologist to be found who will assert the fact, or a logician that will advance the argument, that the encephalon is a useless appendage to the soul; and that she could exercise all her powers as commodiously and effectually in the empty cavern of the skull? Even the best brain which Divine Wisdom could bestow upon her would, in this view of things, be an absurd and superfluous donation.

The third and least obnoxious sense in which materialism may be employed, is that which would consider man as a simple being, whose intellectual powers depend on the peculiar organization in which God has invested them, and regards the resurrection from death as the sole but sure foundation of a future state of existence. This doctrine must be acknowledged by every theologian to derive the most unambiguous support from Scripture;—the physical evidence in its favour is strong and peculiar;—in morals it stands upon the self-same rock whereon we build our hopes of a life to come, come in what manner it may; and possibly it may be slandered in its nature when nick-named materialism.

This opinion I have examined at large in another treatise, which has not yet been submitted to the public; but it is unnecessary here to anticipate the discussions it embraces, particularly as in the essay now under consideration the question does not once occur; and the argument, whether metaphysical or physiological, is pursued with so little bias to or from this opinion, or any other not necessarily involved in the subject, that the disciples of Locke and Berkeley, Priestley and Stewart, may arise from the perusal in perfect amity and good will to the author, satisfied that he has not meant to undermine a single principle, or offend a single prejudice peculiar to any of their schools; unless, indeed, Berkeleyans will complain that the existence of the body, and Priestleyans, that the existence of the soul, is assumed in the argument.

It was, therefore, as unnecessary as unjust to brand my essay with

with the stigma of materialism. So many look to the name and so few to the meaning, that an appellation, supposed to indicate every thing that is contradictory of good sense, inimical to morals, and injurious to society, affixed without discrimination by persons who, in the opinion of the world, never act without exercising their judgement, is sufficient to blast the fortune and fame of the most deserving individual who happens to become the subject of their unwise precipitation. But it is not the individual only who suffers, but the whole body of which these persons are members. What man, capable of an original thought or a bold discovery, will venture to submit his labours to their animadversion, if, in proportion to their novelty, importance and interest, they are to be visited with disregard and opprobrium, in place of the honour they may have earned?

Let me not, however, be understood as casting the slightest reflection on my learned opponents. I am persuaded they acted with conscientiousness, and to the best of their judgement, but from weak and illusory motives. These learned individuals no doubt convinced themselves that it was their duty to oppose to the utmost, the tendency of opinions which their apprehensions conjured into a dangerous heresy, and which they embodied to their imaginations under the frightful appellation of materialism. If they had lived in the days of Copernicus, and were to pass a judgement on his discoveries, with their good will, the rotation of the earth would never have found its way into day-light—rank heresy! it contradicts the Book of Joshua! stifle it in its birth! your duty to God requires the suppression of those truths which most honour him!

No, they reply, these are not truths—for truths must be known by their utility, and utility is not an attribute of heresy and materialism. Such is their argument; but, by a dexterous use of these terms, there is not a truth in physics or morals that might not be easily transmuted into falsehood and crime. I unreservedly subscribe to the dictum of Warburton, that “we may as certainly conclude that general utility is always founded on truth, as that truth is always productive of general utility*.” But short-sighted and ignorant as we are, is it for us to pervade the vast and complicated system of Providence, and decide with formal precision what is to be infallibly useful or pernicious in the administration of the universe? Our business is to discover truth wherever we may have skill enough to find it, and leave it to God to confirm its utility. “In matters (says Hooker) which concern the actions of God, the most dutiful way on our part, is to search what God hath done; and, with meekness, to admire *that*, rather than to dispute

* Warburton's works, 3d vol. p. 225. London, 1811.

what he, in congruity of reason, ought to do* ;” in other words, what we, in our wisdom, shall prescribe to the wisdom of God.

If there is a circumstance upon earth of value in the eyes of Omnipotence, it is the search after truth. Every fresh instance discovered of his ineffable arrangements cannot fail to add a new measure to his glory. If the praises of men are acceptable to him, to unfold the resplendent truths that kindle those praises must be also acceptable. Yet the presumption of pedantry, the ignorance of learning, will officiously thrust themselves forward, and, in hopes of finding favour with God, trample under their audacious feet whatever can do honour to his name.

I have not the miserable arrogance to suppose that the truths I have endeavoured to bring to light are truths such as these ; yet the affectation of humility would just be as miserable if I pretended that I did not consider them of some little value. If I was not satisfied in my conscience that they might fairly claim some small portion of attention, they should never have trespassed on moments too precious to be lavished on vanities and trifles. But I trust I shall not be considered singularly over-zealous in the performance of a duty so important as the vindication of truths neither trivial nor common ; and if it was my duty, it was not less my inclination—I might almost say my passion.—Were I at liberty to change my heraldry and choose anew, this should be my motto—“ Whither Truth leads, thither I follow.”

POSTSCRIPT.—I have lately read an ingenious discussion On the proximate Cause of Sleep, in an Essay by Dr. Park, published in the Quarterly Journal of Literature, Science, and the Arts, for July 1819 ; and I should perhaps have set a more adequate value on his theory, if I had not been already provided with one which, to my own partial judgement, appears somewhat more satisfactory. He designates sleep, as I do, a paralysis ; but he describes it as resulting from a *full* and *slow* circulation of blood in the brain. But I am persuaded Dr. Park will admit that, if this slow and full circulation operates by pressure on the brain, pain, or apoplexy, and not sleep, would be the necessary result. The facts, however, which suggested *his* theory harmonize perfectly with *mine* ; and the full and slow circulation which he establishes, is the very state of the blood in which a deposit of new particles of matter on the brain would be most abundant. The assimilating process, thus in a state of activity, would, according to the foregoing considerations, occasion a paralysis—but a paralysis not bearing any relation to apoplexy, yet sufficiently manifested in the gentler symptoms of sleep.

* Hooker, 1st vol. p. 429. Oxford, 1807, quoted by Warburton, 3d vol. p. 330.

LVII. *On Hypotheses proposed for explaining the Origin of Meteoric Stones ; with Remarks on Mr. MURRAY's Letter on Aërolites inserted in Phil. Mag. for last July. By Mr. H. ATKINSON.*

To Mr. Tilloch.

SIR, — **A**MONG the various natural phenomena recorded in the pages of your interesting Magazine, few, perhaps none, have excited a greater degree of surprise in the beholders, or raised the curiosity of philosophers to a higher pitch, than that of stones having been observed to fall, apparently, from the clouds. A phenomenon so strange, and in appearance so completely at variance with all the known laws of nature, could not fail, on being sufficiently attested, most forcibly to arrest the attention of inquiring minds. Long indeed did philosophers deem it to be a mere popular error. At last, however, such a mass of evidence was accumulated, as commanded the attention of the most prejudiced, and overcame the doubts of the most sceptical. And that stones have really fallen to the earth, apparently from the heavens, however unaccountable it may still appear, is now ranked among established facts. Where they come from, or to what they owe their origin, are questions that have not yet been satisfactorily answered, but which have given rise to much discussion; and, as might be expected in the absence of all direct evidence, various hypotheses have been brought forward to account for so singular a phenomenon. This indirect method of prying into the secrets of Nature by means of an assumed hypothesis, is so very convenient to the majority of mankind, who in general are not very anxious about the correctness of their conclusions, that we cannot wonder at their adopting it. The facility with which it can be applied suits the indolent;—the scope it gives to the imagination pleases the fanciful;—and the opportunities it so liberally affords of attracting public attention, render it a favourite with the vain pretender to a scientific name: but the danger of its leading to error, is a serious objection in the estimation of the inquirer whose aim is truth. Frequently, however, it is the only mode which can be adopted with any prospect of success; and under proper regulations, the risk of its leading to erroneous conclusions may, in many cases, be greatly lessened, if not wholly avoided; so that, in the hands of the judicious philosopher, this indirect method of conducting his inquiries becomes a valuable instrument; a key, that opens to him many of the secret recesses of nature, which, without its aid, must have remained shut up in impenetrable darkness.

When we thus contrast the great advantages that may be obtained by a legitimate use of it, with the gross abuses to which the

the method is liable, it appears surprising that no one should ever have distinctly stated the maxims or rules to be observed in its application to the explanation of natural phænomena. Had this been done, and the rules proper to be observed been generally acknowledged, by restraining the wilder flights of the imagination of the visionary, and by repressing the impertinent intrusions of the vain or the ignorant upon public attention; it would probably have greatly lessened the number of those chimerical or absurd hypotheses, which have such a tendency to bring disgrace upon a useful instrument of investigation, and which are a grievous tax upon the time and patience of the reader.

The more immediate cause of these observations is the vague and inconclusive mode of reasoning adopted by many, when attempting to discover the origin and to account for the phænomena attending the fall of meteoric stones; and in this respect, few, if any, of their predecessors have exceeded some of your late correspondents on this subject. Curious as it undoubtedly is, and therefore interesting as the subject must be to many, it does not however appear to be a matter of any very great importance to mankind in general, whether these bodies are supposed to be occasional visitants from celestial regions, or are imagined to be "children of the air," or whether we ascribe to them a still more humble birth, and acknowledge them to be of terrestrial origin; but it is always of great consequence to society to preserve any useful mode of investigation from such gross abuses as would bring it into disrepute, and the frequent repetition of which must have a strong tendency to introduce a vague and sophistical manner of reasoning. It is on this account that I am induced to offer a few remarks on a letter from Mr. J. Murray, on *Aërolites*, published in the *Phil. Mag.* for July last, in which that gentleman tells us, that he "read with some degree of astonishment Mr. Brande's opinion on the origin of meteoric stones," because he "believed their supposed lunar origin had been generally abandoned, and that the opinion which confined them to our atmosphere had ceased to be problematical." Now from this, one would naturally expect that Mr. Murray had some mode of accounting for the formation of meteoric stones, which was, at least, plausible;—how far this is the case I shall take the liberty of inquiring.

His first assumption is, that hydrogen and oxygen gases are capable of dissolving or combining with all the ten different substances which are occasionally found in meteoric stones. And the reasoning, if such it may be called, by which he supports this assumption, is as follows: "Hydrogen dissolves iron and sulphur. It may perhaps be capable of dissolving other two, viz. silica and nickel, although it has never yet been found to have

such a power." And he then very sagely concludes, that we have no right to limit its solvent powers. With respect to the remaining substances, he disposes of them in a very laconic manner; thus, "As for oxygen, &c." says he, "for any thing I know, they may have very exalted solvent powers." And for any thing I know, Mr. Murray may possibly, nay probably does, believe this to be quite sufficient to establish his assumption on so firm a basis that it shall cease "to be problematical;" for he advances nothing further in its support. Such proof, however, does not appear to require any comment.

His next assumption is, that there are two immense aërial volumes, loaded with the requisite materials, floating either in or on the atmosphere: one he supposes to be oxygen, and the other hydrogen. These, he says, "would be *differently* electrified; for oxygen with its contained materials, and hydrogen with its accompaniments, would certainly be so:" but how he arrives at this certainty he does not deign to tell us. Can Mr. Murray have discovered the secret of determining what effects two bodies will have upon each other, without knowing the properties of either? for unless he can do so, it is not very easy to conceive how he could arrive at this certainty; as neither he nor any one else knows any thing about the properties either of the "oxygen with its contained materials," or yet of the "hydrogen with its accompaniments." We do indeed know some of the properties of oxygen and hydrogen gas; but we have every reason to believe that the "contained materials" of the one, and the "accompaniments" of the other, which Mr. Murray alludes to, would change these properties; and what that change would be, we have no means of determining; nay, we do not even know that they would continue in a gaseous state. Neither does he inform us how these two immense aërial volumes are collected. He does indeed say that "hydrogen variously combined is continually escaping from all parts of the surface of the globe:" and again, "the combined hydrogen might in virtue of its great levity, and expanding as it ascended, finally brave the outer circle of the atmosphere and settle upon its waves." But here we may be allowed to inquire how Mr. Murray knows that hydrogen thus combined is of such extreme levity:—does he draw this conclusion from its combination with sulphur? Waiving this objection at present, let us suppose that the hydrogen and all its accompaniments are mounted as he imagines; still, however, we are far from being done, our task is not half finished; for we have yet to obtain the "oxygen with its contained materials," to collect it into one immense volume, and to transport it to the outer circle of the atmosphere, there to settle upon its waves: or else we must drag down the "hydrogen with its accompaniments" from its lofty throne, and obtain
separate

separate lodgings for them in the atmosphere. But by what vagary of the imagination these things can be supposed to be accomplished Mr. M. has not told us; he has not given us so much as a single hint how to proceed, but has left us entirely to our own resources; and, considering the creative powers of this gentleman's fertile imagination, and the difficulty of the undertaking which he has imposed upon us, it does not appear very handsome thus to leave us in total darkness.

However, lest it should be imagined that we object to trifles, let us suppose that all these difficulties are overcome; yet even in this case our labours are far from being ended; for after we have got the two gaseous solvents collected, and mounted far beyond the clouds, or accommodated with separate chambers in the air, we have still an arduous task left; they must be kept from mixing with the atmosphere, till some favourable circumstances bring them into contact. But how this is to be done is not so easily perceived: for it is well known that all kinds of gases, yet discovered, diffuse themselves through atmospherical air whenever they come in contact with it, even when kept perfectly still;—how much more quickly then must they be mixed in an agitated atmosphere! And as the quantity of either oxygen or hydrogen which has ever been known to issue from the earth, is so very trifling when compared with the surrounding atmosphere, they must, in every case, be completely diffused in it: nay, even in the extreme case of a volcano sending forth a quantity of hydrogen gas, it would in a few days be so diffused, that it is very doubtful whether it would amount to such a quantity at any one place as to be appreciable by the most accurate methods yet known. This is a circumstance which Mr. Murray ought to have been very careful in guarding against, as an oversight here must prove fatal to his hypothesis: for if the gases get mixed with the atmosphere, the mixture would be far too weak to be capable of ignition, even if we suppose the different states of electricity still to remain, and the electric explosion to take place.

As Mr. Murray says that in hypothetical cases “we are at liberty to suppose what we will,” I shall avail myself of this liberty, by supposing, however improbable or absurd the supposition may be, that all the foregoing objections and difficulties are not worth noticing, and that his hypothesis is still admissible and entitled to our serious consideration: yet even this gigantic effort in its favour will avail but little: for the consequences which he asserts would follow, could not possibly flow from his premises. He says, “The two electricities rushing into contact would produce explosion; the gases would be ignited, the stony materials undergo fusion,—and in that moment the formed *aërolite* would take its

flight to the earth." Now, according to his own statement, the gases could never be ignited: for if the hydrogen and oxygen gases were in separate volumes, the passage of the electric spark from one to the other could inflame neither, each being incapable of ignition when alone; and if they were either mixed or in contact, no electric spark could be exhibited, as the electric fluid would then pass silently from one to the other, and consequently there could be nothing to inflame the mixture; so that this alone would be sufficient to overturn the hypothesis.

But let us again have recourse to our liberty of making what suppositions we please, and suppose that the gases would really be ignited: does it from thence necessarily follow that the stony materials must undergo fusion? or, if they do, that they must therefore be aggregated into one solid mass? These are certainly far from being self-evident consequences; so far, indeed, that they appear altogether improbable: for instance, if sulphuretted hydrogen and oxygen gases be brought into contact and inflamed, so long as the quantity of the latter is either considerably less than that of the former, or supplied slowly, a great portion of the sulphur will be deposited, unchanged, in the form of a fine diffused powder. And if such an inflammable body as sulphur be not ignited, how very improbable it is, that such stubborn materials as iron, alumine, and silica should undergo fusion! Should it, however, be alleged that the sulphur is actually fused in this experiment, but, on account of the greater affinity of hydrogen for oxygen, it could not become ignited for want of the latter; it must then undoubtedly follow, that fusion does not necessarily lead to the aggregation of the different parts even of the same substance, much less then of heterogeneous materials. Hence it is very improbable that the stony or metallic parts would undergo fusion; and even if they did, it would be altogether inadequate to account for the aggregation of such different materials into one mass: for we know that all chemical depositions from the explosion of gases are in the form of fine powders.

One circumstance attending the fall of meteoric stones seems to have been overlooked not only by Mr. M. but by almost every one who contends for their atmospherical origin; and yet it is a difficulty of no common magnitude;—indeed the obliquity with which they fall seems altogether unaccountable on any known principles, if they be generated in the atmosphere. The electric fluid has by some been supposed to be the moving power in this case; but it appears to be incapable of communicating such a velocity. In the most tremendous thunder-storms it never removes any thing from its place more than a few yards; it therefore cannot be this power which communicates their horizontal velocity to
meteoric

meteoric stones after they are formed in the atmosphere. Neither are we acquainted with any power which could give such a horizontal velocity to the different materials while in their diffused state, or while held in solution by some gaseous fluid. Electric attraction might indeed give a slow motion to them; but it is quite incapable of giving such a velocity as to account for the phenomenon in question; it may perhaps, in very favourable circumstances, be able to produce a horizontal velocity of about thirty or forty feet per second; but this is very far short of what meteoric stones must have at the moment they are formed. The only power in nature, with which we are at present acquainted, besides electricity, that appears to be able to give any considerable horizontal velocity to a solid mass formed in the atmosphere, is the expansive force of inflammable gases when exploded. We know that the rapidity with which several mixtures of this kind expand at the moment of explosion is very great; some of them probably at the rate of several thousand feet per second. Let us therefore inquire whether this can be the source of the horizontal motion of meteoric stones.

It is well known that some of these stones have fallen so obliquely, and with such force, that their horizontal velocity could not be less than 200 feet per second, when they struck the ground. But it is evident that the original horizontal velocity of any meteoric stone must be very much reduced by the resistance of the air before it reaches the earth: thus, for example, if we suppose a stone to be spherical, to weigh 100lbs. and to have been formed at the height of four miles; it will be found by calculation that it would require to be projected with a velocity of considerably more than 2000 feet per second, that its horizontal velocity when it reached the ground might be 200 feet per second. Again: air at the height of four miles is about 1870 times lighter than water, and the stone being $3\frac{1}{2}$ times heavier, it follows that the stone must be above 6500 times the weight of an equal bulk of air at that elevation. Now every meteoric stone must be formed very near to that point which is acted on equally in all directions, by the expansive force produced by the explosion: and when we consider that the horizontal velocity can only arise from the *excess* of the force applied on one side over that which is applied on the others, how enormously great indeed must the rapidity be, with which this elastic gas or vapour expands itself, if only a small portion of its force can communicate a velocity of more than 2000 feet per second, to a body 6500 times as dense as itself, and that too in a situation completely unconfined! It will not be sufficient that it expand itself with a velocity of 7 or 8000 feet per second; no, not even fifty times 7000 would be enough. Hence it ap-

pears, that any hypothesis which ascribes an atmospherical origin to meteoric stones, necessarily implies, that the horizontal velocity is either communicated to them by something of which we cannot at present form even a conception; or else, that there exists in the atmosphere a projectile power of enormous, nay of almost inconceivable force; so great, indeed, that the power required in the lunar hypothesis is a mere trifle to it: and yet Mr. Murray describes the latter as being "of the most extraordinary description," and one "not merely of enormous, but of almost inconceivable impetus;" while he seems to think "that the opinion which confines them to our atmosphere," although it really requires a projectile power at least fifty times as great, is attended with so little difficulty that he believes it has "*ceased to be problematical!*" and seems quite astonished that Mr. Brande should be of opinion that the lunar hypothesis "is, when impartially considered,"—what? not at all problematical? no; merely this, that it is—"neither *absurd* nor *impossible*."

This letter has already extended to a much greater length than I intended; yet I cannot close it without observing, that the reasoning which Mr. M. has employed in opposition to the lunar hypothesis, is almost as strange as that which he has used in support of the atmospherical theory; but at present I have not time for a full examination of it: the essence of it may however be given in a few words; and the bare exhibition of it as it really is, when stripped of its gaudy trappings, will probably be sufficient to show its absurdity.

He thinks it unphilosophical merely to suppose a thing to be, which most philosophers believe really to exist; viz. lunar volcanos.

Because terrestrial volcanos have not power sufficient to project a body with the enormous velocity of more than 100,000 feet per second, he thinks it unreasonable to suppose that lunar volcanos may be able to propel a body with a velocity of about 7000 feet per second, although this is little if any thing more than what a sufficient quantity of our gunpowder would produce at the moon. And lastly, because he chooses to ascribe consequences to the lunar hypothesis, which do not belong to it, we must therefore abandon it. I am, sir,

Your most obedient servant,

Newcastle-upon-Tyne, Oct. 16, 1819.

H. ATKINSON.

LVIII. *Reply to Mr. EDWARD RIDDLE's Remarks on Mr. MEIKLE's Paper "On finding the Longitude by Lunar Observations."* By Mr. MEIKLE.

To Mr. Tilloch.

SIR, — **I**N your last Number, Mr. Riddle complains grievously of a paper of mine containing miscellaneous remarks on the lunar observations, which you were kind enough to insert in your Number for July. He with no small labour pretends to give an entire refutation of it; but in no instance has he succeeded in treading down any material point, by his very unfair and disingenuous examination of the whole.

One thing in particular which seems to have alarmed his jealousy, is the apparent levity of the style in which I delivered myself: but every person is not blessed with a style possessing all the gravity and worth of Mr. Riddle's. Another defect is my want of candour and moderation; but apparent candour and moderation are often veils to loathsome flattery, while truth and honesty are wanting. And it even sometimes happens in our golden age, that persons are loud in their praises of "candour and moderation," when they are about to commit an outrage against both.

I have of late, it is true, given my opinion on various subjects in the public journals, without attending to Mr. Riddle's precaution of presenting a peace-offering at the commencement. No, I deem it more honest and honourable, nay, even more respectful, just to make known my real intention at once without dissimulation. I rejoice at all times to see errors, even mine own, corrected; nay, in that very paper itself, so unjustly censured, I have readily acknowledged my liability to err, but have not yet found that it contains any thing amiss.

Mr. R. further observes, "that a perusal of that letter will show those unacquainted with the subject, the spirit in which it is written." Now this is just the thing. It is only such as are unacquainted with the subject who will take offence. Others have no reason to be alarmed—they are not accused. I mentioned no names, but left the guilty to apply it to themselves.

But that the reader may see that I am not alone, nor yet the first, to make free remarks on writers of navigation as well as on the deplorable state of mathematical knowledge in general; I beg leave to refer him to Dr. Mackay's preface to his "Navigation," where many of these worthies are made black enough.—See also Dr. Gregory's preface to his "Mechanics." In short, there are few scientific authors who do not make similar complaints; and generally they will be found to make use of much severer censure than I have ever done. It is the sad reproach of our nation, as well as of neighbouring states, that my animadversions are too true. The

order of the day is not science, but gambling and infidelity, twin-sisters of ignorance, with all their kindred consequences.

I readily grant that we have some sterling writers on Navigation; but it is also too well known that sorry pretenders, totally unacquainted with the elements of science, take upon them to compile books of navigation. Nay, it has been the dismal lot of respectable works to fall into the hands of proficients in ignorance, who have republished them "revised and corrected" in a form every way worthy of themselves. One thing which made me more severe on such authors was, that they are so much in the habit of giving distant approximations without the least intimation that they are not theoretically correct. Indeed, so very generally is this the case, that I have often met with persons who, like Mr. Riddle, would hoot and sneer at my ignorance, if I only hinted such a thing. This therefore makes me less ceremonious in delivering my sentiments.

At present I do not intend to follow Mr. R. through all the shifts and stratagems to which he has been forced, in order to put down truths which rest securely on their own bases; but shall merely point out some of the most glaring, reserving the rest till another opportunity.

After giving an example of correcting the moon's altitude, on page 244, Mr. R. gravely consoles himself, saying, "This I hope will be satisfactory." Now, I say it is most unsatisfactory, as well as unfair and unjust: for I did not deny that with plenty of needless labour, which Mr. R. has kindly concealed, the altitude of the centre might be had true in that way. But the peculiar superiority and unrivalled excellence of my method (as he calls it) is, that by it the true altitude may be had correct with the least possible labour, two corrections being thereby entirely avoided. It is on this account that the latter method is always employed in reducing the moon's place, for the construction as well as for the correction of the *lunar tables*. But for all this, and "though sanctioned by the authority of such names as Maskelyne, Pond, and Brinkley;" yet persons like Mr. Riddle, who are faithfully wedded to their old established habits, cannot be induced to adopt it.

To the scientific reader, my remarks on the quadrant must appear perfectly reasonable and correct. Mr. R. has not been able to show the contrary, when he has favoured us with the explanation which is to be found every where. Indeed, so scanty is the mathematical knowledge of most persons into whose hands quadrants are put, that we can never be too simple in our explanation; and it must be admitted on all hands, the most simple explanation is always the most scientific. Was Mr. R. afraid that by simplifying the explanation, persons might make them-
selves

selves masters of the whole mystery, so that by and by there would be no use in going to school, and neglect to attend school?

Dr. Hooke's quadrant with one reflector was quite correct in theory, and had precisely the same property of halving the angle. Indeed we would not necessarily in theory alter that property, by increasing the number of fixed reflectors; for this very good reason, that it has no dependence upon them at all.

Mr. R. afterwards accuses me of ignorance of the true definition of parallax, and out of his great goodness and compassion informs me what it is. But in order to show which of us has the most correct idea of it, I beg leave to adduce Professor Playfair's definition, as certainly of higher authority than any pedantic definition invented for a particular purpose. It is as follows: “The *parallax* of any object in the heavens is the difference of its angular position as it would be seen from the centre of the earth, and as it is seen from a point on the surface.” (Nat. Philosophy, vol. ii. art. 74.) We are therefore at liberty to take any point of a heavenly body for our object of observation; as for instance, a point in the boundary of the lunar disk, as I have done. Your readers will readily see that Professor Playfair understood parallax precisely in the same “unusual sense” as I do; since he makes no mention of the *centre* of a heavenly body having more to do with parallax than any other point. Indeed the very circumstance of the impracticability of applying an instrument to the centre, puts that out of the question; and besides, our most eminent practical astronomers always apply the correction for parallax to the limb, and not the centre. Of this I certainly stand fully as much in the way of being correctly informed as Mr. Riddle can; had he thought of that in time, he certainly would never have coined his counterfeit definition, the currency of which cannot go beyond the walls of his own school.

I formerly stated that the difference of the parallaxes for any *two* diametrically opposite limbs constitutes the augmentation of the diameter; and by any *two* such limbs or extremities of a diameter, I am still disposed to abide*. In order to refute that statement, Mr. R. with great ingenuity has done for me what I could not; he has invented the learned absurdity of a case with *four* limbs D, E, F and G (page 248); and then shows his very superior skill in refuting his own favourite delusion. Does he suppose I shall remain silent whilst he would father his own

* In order to show the truth of this, it is only necessary to consider, that by applying the correction for parallax to each extremity of the diameter, we bring that diameter into its proper place, and of course reduce it to its proper length. But since the change of length is manifestly the difference of the parallaxes reckoned in the direction of that diameter, it follows necessarily that the difference of the parallaxes is the augmentation of their diameter.

four-footed progeny on me? If it be true of *two* limbs, it is no concern of mine though it were false of fifty.

Indeed the only shadow of objection that can be brought against my statement is, that the bounding circle of the lunar disk is rather nearer the observer than the moon's centre is. But such an objection, which he was not likely ever to think of, would be perfectly ridiculous. We might next talk of the effect of a lunar volcano. Laying then that frivolous objection out of the way, I am fully warranted to repeat, that the difference of the parallaxes of any *two* diametrically opposite points is the augmentation of that diameter. I say, that diameter itself; because the augmentation is not the same for every diameter. For I must take the liberty of informing one who arrogates to himself so much superiority over me, that although, as I said before, the apparent disk of the moon is not rendered elliptical by parallax; yet when every bounding point of that disk has been corrected for parallax, the resulting figure is an ellipsis whose transverse axis is parallel to the horizon. This will be clear to every unprejudiced person, when he considers that the bounding circle of the apparent disk as seen from the earth's surface, is an ellipsis when viewed obliquely from the earth's centre: and it is with this one circle that we have to do.

The correction called the augmentation is therefore greatest in a vertical direction, that even existing at the horizon in a certain sense; and on this principle, perhaps, might the augmentation be computed, more free from theoretical objection than any other equally simple. Mr. R. is not likely to relish this doctrine. He will certainly dismiss it with a sneer of sovereign contempt, as he kindly did the "angular point of the triangle;" because he could not offer one mouthful of rational argument against it.

He would be thought, no doubt, to construct the figure on page 248, as if he had viewed the moon from the earth's centre. But will any one who looks at that most absurd diagram, believe that Mr. Riddle was any lower than the bottom of a Newcastle coal-mine, where he would be infinitely less in danger of a fall than if hovering in the air "over London-bridge"?

With regard to Dr. Mackay, and some others, not having noticed the first-mentioned defect of the correction of latitude, we have no evidence to the contrary. But in order to show that even Mackay himself, whom I nevertheless highly esteem, was liable to similar mistakes, (and who is not?) I beg to refer the reader to page 353, vol. i. of his *Treatise on the Longitude*, where he will find a very erroneous rule for a correction of the azimuth, arising from the change of declination during the time elapsed between observed equal altitudes. The very same thing, illustrated by an example, occurs in his *Navigation*. Others perhaps have

copied

copied it into their works in its original inaccurate state. It should be—To log. secant latitude, add log. cosecant hor. angle and log. change of declination; the sum will be the log. of correction of azimuth, &c.

I may perhaps give the investigation afterwards. We can never be too much on our guard against error.

I am, sir,

Your most obedient servant,

Berners-street, Nov. 2, 1819.

HENRY MEIKLE.

P. S.—Mr. Tredgold has by no means refuted my objections; but I cannot now attend to his case.

LIX. *A Description of a new Military Bridge, that may be made of short Pieces of Timber, and easily put together in any Situation. The same Method is applicable to other Uses.*
By A CORRESPONDENT.

To Mr. Tilloch.

SIR, — I HAVE called the combination of timbers now to be described a military bridge, not because it is peculiarly adapted to that purpose, and that purpose only, for it is equally applicable to any other portable bridge, or other erection of a temporary nature, but because as a military bridge it would be most useful; and whoever considers the difficulty which the passage of a river or canal sometimes offers to an army, will allow me, without censure, to offer a hint that may be of use on such an occasion.

There are some who will object to the introduction of such a subject at the present time: to these it will only be necessary to state, that the education of engineers must not be neglected, even in peaceful times, unless indeed we should intend to become an easy prey to some other nation, or to call in the assistance of a more warlike race. Britain, I hope, will never be reduced to such a state, through the neglect of cultivating those sciences and habits which constitute the strength of nations.

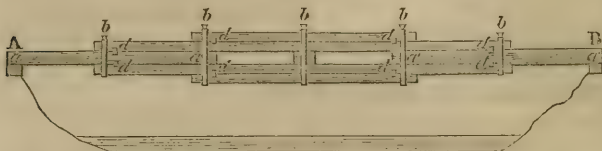
Lately, the construction of military bridges has been very ably treated in an Essay by Col. Sir H. Douglas, who has in that work described a simple combination of much merit, which may be extended to a 26 feet span*. In order to span either a 50 feet opening, or any smaller one, I offer the following method.

Let two or more frames be put together, as is shown by the sketch in 348. When these frames are placed across a river, or other opening, with cross-bearers notched upon them, and planks for

* Essay on the Construction of Military Bridges, p. 164.

a road-way laid upon the cross-bearers, the assemblage would constitute a bridge sufficiently strong for many temporary purposes.

Let us suppose the span to be 50 feet, then the longest piece of timber required would be about 18 feet, or a little more than 1-3d of the opening, and the sketch shows the method of disposing the pieces.



For a 50 feet span, the end pieces *a a*, *a' a'* ought to be about 9 inches deep and 6 inches in breadth, and 18 feet long. The other pieces of the same length, but only 6 inches square.

C is a projected sketch of one of the iron straps at *b, b, b, b, b*, in the preceding sketch. When these straps are put to their proper situations, they are tightened by a screw-wrench applied to the screw *c*.

In fig. *AB*, the letters *d, d, d, d, d, d*, show where pieces of hard wood should be let into the joints to prevent any sliding of the parts when the bridge is loaded.

A bridge consisting of two frames, such as *AB*, over a river 50 feet wide, would bear the weight of infantry marching under a front of two men, files close; four frames a front of four men; and so on.

If Riga or Memel timber be used, the heaviest pieces will not weigh more than about 140 lbs. each; so that two men may carry one of them with ease; and from the nature of the fastenings proposed, a frame may be put together or taken asunder with the utmost facility, without injury to the materials.

In cases where the proper materials have not been provided, ropes might be employed instead of straps of iron; these ropes might be drawn perfectly tight by means of short staves for twisting them; and such timber might be employed as the place afforded:

With the assistance of floats, and the force of the current in a river, it would be easy to get the frames to their intended situation; but with expedients necessary for accomplishing this object, military men are in general likely to be familiar:—if not, the excellent work of Sir H. Douglas will supply the necessary information.



formation. For other uses, where expedition is of less importance, it will be easy to place the frames in the position desired.

It may not be amiss to point out a case or two in civil life where the combination may be useful. In scaffolding, where intermediate supports cannot be applied from the nature of the situation; in temporary bridges; also to roofs and other parts of temporary buildings, or erections. Let us suppose that a large room is to be erected for any purpose. If it were 60 feet wide, it might be covered, with the help of the method now pointed out, in a very short time, and almost without waste of materials; besides the advantage of employing short pieces, almost without preparation, it merely being necessary that there should be some pieces of the same depth.

In conclusion: I shall make a few observations on the principles which were kept in view in contriving this combination of timbers. In the first place, it is well known to those who have considered the nature of beams of equal resistance, that when a solid beam, supported at its ends, is acted upon by a moving load, its vertical section in the direction of its length is an ellipse, when it is equally strong at every point. The combination above described was intended to approach as nearly to that form as a necessary attention to other circumstances would admit. Those who have not had occasion to study this part of mathematical science, may most likely have observed that a beam may be much reduced towards the points of support without impairing its strength.

Secondly. When a beam, resting upon distant supports, is strained by a load upon any part of it, the fibres at the middle of its depth are not strained in a sensible degree; the strain being greatest at the upper and lower sides, and decreasing to nothing in the middle. Hence the middle part may be removed without materially lessening the strength, at the same time the load upon the beam is much lessened. Thus, combining strength and lightness, the stems of the reed and many other plants owe their strength to the same principle. And a still more striking example is exhibited in the bones and wings of the feathered tribe. It is one of those remarkable cases, where through science we seem to look into the process of nature: but how clumsy are our attempts to imitate her works!—Nevertheless it is always possible to gain much by a due attention to phenomena which are daily before us; and as our knowledge increases, the more we find to admire—to imitate—even our own want of skill must often increase our veneration for Him, whose works fill our minds with awe and astonishment.

CIVILIS.

LX. *Me-*

LX. *Memoir on a new and certain Method of ascertaining the Figure of the Earth by means of Occultations of the fixed Stars.* By A. CAGNOLI. *With Notes and an Appendix by FRANCIS BAILY*.*

ADVERTISEMENT.

The following memoir originally appeared in the Transactions of the Italian Society (*Memorie di Matematica e di Fisica della Società Italiana*, Tom. vi. Verona 1792): but, although it has been so many years before the public, I do not find that the subject of it has been taken up by any persons either here, or on the continent. In fact, I believe that it is not generally known in this country: and it is with a view of drawing the attention of astronomers more closely to it, that I now present them with this translation. The acknowledged talents and abilities of the author must at all times ensure it an attentive perusal: but particularly at this period, when a more than ordinary degree of interest is excited towards the subject of the *true figure of the earth*. I am aware that there are some apparent difficulties in the method pointed out in the memoir, yet I am not without hope and expectation that they may be eventually overcome: and that the mode herein proposed may at least be brought in aid of the other methods at present adopted for determining that difficult problem.

To the memoir itself I have subjoined some notes connected with the subject; which, however, may be distinguished from those of the author by the addition of the letter B. And to the whole I have added an Appendix, wherein I have attempted to illustrate the views of the author; and ventured to propose such methods as may best tend to carry his object into execution.

Gray's Inn, May 20, 1819.

FRANCIS BAILY.

MEMOIR, &c.

§ 1. **T**HE truth of the Newtonian theory of universal gravitation is proved by the wonderful agreement in all the celestial phenomena, which have hitherto been submitted to the tests of calculation and of observation. There probably is not any astronomer of the present day who is not convinced that our planet is compressed, or flattened, at the poles, and protuberant at the equator. In fact, if we combine this theory of Newton with the hypothesis of the elliptical form of the earth, we shall find that the precession of the equinoxes and the nutation of the earth's axis are sufficiently accounted for. Other arguments indeed, in favour of the elliptical form, may be derived from the rotation of the earth, which is now generally admitted: and others again from the differences in the length of the pendulum vibrating seconds in different latitudes. Still, however, we want some theory of the internal density of the earth; which, it is feared, will ever remain hid from human investigation. But if, to the want of

* The present memoir has been already printed, but not for sale. It is now reprinted with the polite permission of the author.

this essential knowledge, we add the irregularity of the figure of the earth, as shown by the measurements of the degrees on its surface ; if, to the doubts which some persons may still entertain of the truth of the compression of the poles*, we add the discordant opinions as to the quantity of that compression ; we may surely assert that it will be no small advantage to point out a method (independent of all hypothesis) for determining, with facility and with the greatest precision, the differences between the terrestrial radii at an indefinite number of points on the earth's surface.—Such indeed is the object of the present memoir.

§ 2. It has always been said, when speaking of the compression or flattening of the poles of the earth, that the *parallax of the moon* would afford the best means of ascertaining it ; provided the variations, arising therefrom, were of sufficient magnitude to be observed with perfect accuracy. But, since, by supposing this compression not to exceed $\frac{1}{360}$ of the semidiameter (which is the most received opinion at the present day) there would be a difference of not more than 9'' between the moon's parallax at the equator, and her parallax at a high latitude, for instance 60° ; so indeed it is but too true that a difference so small might be easily concealed under the possible errors of observation. It was on this account that Manfredi and Maupertuis in vain suggested the determination of the compression by direct observations of the moon's parallax : nor has any advantage been hitherto obtained by this method.

§ 3. But this is not the only instance in which a quantity, extremely minute on one side, has been found to produce effects that are quite discernible on the other. The attention of the observer should be directed to those points where they are most sensible. This has been the object of my research ; and I hope not entirely without success. For, there are circumstances in which scarcely a *second* in the parallax may cause a difference of 15, 20, or 30 seconds of time, or even more, in the duration of the occultation of a star by the moon. But, occultations, more particularly when the immersions take place behind the dark limb of the moon, can easily be observed without committing an error of a single second of time † : so that opportunities exist not only of removing all doubt as to the reality of the compression of the poles, but also of such frequent occurrence that the gradation of the compression, (or variation of the curvature) at different lati-

* See Lorgna "*Principj di Geografia &c.*" § 31.

† The duration of an occultation cannot probably be so well observed when the immersions take place behind the dark limb of the moon, as when they take place behind the illuminated side ; since the *instant of emersion* cannot be so well ascertained. B.

tudes, may be accurately distinguished. The importance of the subject induces me to treat it more fully in detail.

§ 4. I shall not take up the time of the reader in demonstrating that no hypothesis is required to deduce the variation of curvature from the observation of the parallax. For, since the variation of curvature is nothing more than the inequality of the terrestrial radii (and that is nothing else than the difference of the parallax), it is evident that finding by observation the variation of parallax at different latitudes is in fact finding the variation of curvature by immediate observation. One involves the other without any intermediate help. The only difficulty consists in freeing from uncertainty those observations which show the unequal parallax: and this is precisely the special character of the particular kind of occultations which I have in view. It is true that astronomers, in calculating occultations, have made the parallax vary in conformity with that quantity of compression which they have adopted as the true theory. Moreover, these variations do not in general produce differences which are discernible in the relative duration of the occultations observed in different latitudes. Those differences, which *are* discernible, arise only in certain circumstances, to which hitherto no particular attention has been paid. It is on this account that occultations have not, as yet, served at all to determine the amount or quantity of the compression of the polar axis.

§ 5. Before I proceed to describe what these favourable circumstances are, it will be proper, in order to appreciate their utility, to define the precise degree of accuracy of which the *computation* and *observation* of occultations are capable. With respect to the *computation*, it is evident that the apparent distance between the star and the moon cannot be accurately determined unless we know exactly not only the apparent place of the star, but also that of the moon and its diameter. These elements are obtained by calculating the phenomena from observations made in a place (the longitude of which is known) under circumstances where the alteration of the parallax, on account of the variation in the curvature of the earth, does not produce a perceptible difference in the duration of the occultation: conditions not uncommon, nor difficult to be obtained.

§ 6. Let us then suppose that the moon's diameter and the place of the star are well determined (in which elements the least uncertainty exists); and that any small errors, which may occur, be thrown on the place of the moon. Let us, moreover, endeavour to discover (by comparing the calculation with the observation) the errors of the tables in the longitude and latitude of the moon, mixed (as we have already stated) with the preceding errors.

errors*. In the present state of astronomy I do not see any great danger to be apprehended from this union of the two errors, notwithstanding the delicacy of the investigation in question. At any rate we may arrive at a sufficient degree of accuracy by verifying the place of the star by a sufficient number of observations, and also the diameter of the moon by means of the telescope which is used for the occultations.

§ 7. Having corrected the errors of the lunar tables, we shall have accurate elements wherewith to represent the state of the heavens: and there can then be no doubt that we may arrive at great exactness in our calculation of the duration of an occultation (or of the moment of immersion and emersion) for the place in which we wish to ascertain the curvature of the earth: since the geographical longitude of the place has been determined by means of occultations under circumstances in which (and they are the greater part) the variation in the curvature is not perceptible.

§ 8. With respect to the accuracy of observations of this nature, I have already said that the immersions behind the dark limb are not subject to the least error. Those on the illuminated side may be liable to some slight uncertainty if the star be not of the first or second magnitude, or if the power of the telescope be too small. The emersions from the dark limb are in general to be depended upon: whilst those from the illuminated side are the most doubtful of all†. But, even supposing that the error, in the last-mentioned case, may amount to 8 or even 10 seconds of time, will it be sufficient to conceal the variation of curvature altogether in the circumstances I have contemplated, and where the difference that it will produce may be ten or twenty times greater than the presumed error? Can we, indeed, expect to obtain more certain observations‡? since a single observation

* See the Memoir which obtained the prize from the Academy of Copenhagen: "*Méthode pour calculer les longitudes géographiques.*" Vérone: chez Ramanzini.

† It appears that, in some occultations, the immersions and emersions take place wholly on the dark side of the moon. See an account of the occultation of β Virginis observed by Mr. Troughton on May 22, 1801. *Connaissance des Temps, Année xiii.* page 324. Sometimes an occultation may be observed when the moon itself is not visible: as in the occultation of Venus on May 13, 1801, between 8 and 9 o'clock in the morning; the moon being then only a few hours old. *Ibid.* page 417. A favourable occultation may also occur during a total eclipse of the moon, when the immersions and emersions of stars of the 6th or 7th magnitude may be distinctly observed. *Ibid.* Année ix. page 335. B.

‡ I find it difficult here to give a faithful translation of the author's words: the original runs thus, "Senza che ci possiamo attenere alle osservazioni più sicure: ed una sola fase, &c. &c." B.

of this kind accurately made in the place, for which the variation of curvature is sought, is sufficient to discover it by means of the comparison of the *calculated* with the *observed* moment.

§ 9. I shall now point out what are, in fact, the circumstances in which the differences of the parallax between two latitudes may be shown in an undoubted manner: and, in order virtually to embrace all the cases, I shall take mean quantities in the elements which enter into this investigation. Let us therefore suppose, first, that the moon's apparent semidiameter is $15'.45''$, its equatorial parallax $57'.40''$, and its horary motion $32'.56''.5$: secondly, that the occultation is observed in north latitude 60° , in which parallel there are three celebrated observatories, viz. Petersburg, Stockholm and Upsal: lastly, that the apparent height of the moon is 10° . If there be no variation in the curvature of the earth's surface the parallax of height will be $56'.47''.4^*$: but, if the polar compression amount to $\frac{1}{300}$ of the earth's radius, the same parallax will be $56'.38''.9^\dagger$. The parallax would therefore under these circumstances experience an alteration of $8''.5$; a quantity certainly too small to be verified with accuracy by means of observations of the height of the moon. But, this slight alteration produces, in certain cases, effects that are very visible: a fact which escaped the observation of Maupertuis; who, it is true, speaks of occultations as a mean of discovering the flattening of the earth $\dagger$. But, he speaks of them generally; and so slightly as to class them with appulses, as being *equally* fit to determine it. Now, as appulses are far from being observable with such certainty, in regard to time, as the instantaneous disappearance and reappearance of stars in occultations, it is evident that Maupertuis can never have had in view the particular cases which I am about to point out, and which differ materially from ordinary ones.

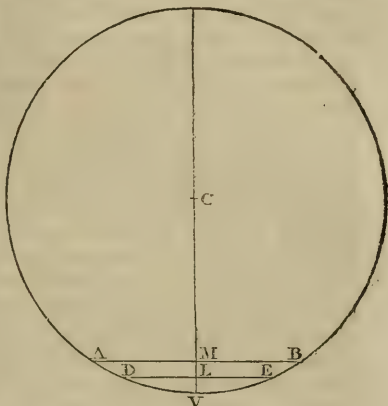
* Let p = the horizontal parallax of the moon; and h = the height of the moon: then $\pi = p \cdot \cos h$ = the parallax of height. B.

$\dagger$ Let a = the polar compression of the earth, supposed = $\frac{1}{300}$; λ = the latitude of the place; and the radius of the equator equal to unity: then we shall have the length of any other terrestrial radius = $(1 - a \cdot \sin^2 \lambda)$ nearly; which, being multiplied by π , will give $\varpi = \pi (1 - a \cdot \sin^2 \lambda)$ = the parallax of height on the supposition that the earth is an oblate spheroid. Whence we also have $\frac{\varpi}{\pi}$ = the length of the terrestrial radius at any given latitude. And if δ denote any given increase of the parallax, we shall have $\frac{\delta}{\pi}$ for the corresponding increase in the length of the earth's radius. B.

$\ddagger$ *Préface au discours sur la parallaxe de la lune.*

§ 10. Let us suppose AVB a portion of the circumference of the moon's disc opposite to the observer; C the moon's centre; and that the radius CV, which divides the arc AVB into two equal parts, coincides with the vertical circle of the place of the observer.

Let us further suppose that the line MV (or the ver-sine of the arc) is equal to $60''$; and that, during the occultation, the chord AB of the moon is that which the star would apparently describe if the earth were spherical; or the chord DE that which it would appa-



rently describe if the axis of the earth were compressed $\frac{1}{360}$. Then ML will be equal to $8'',5$ as before mentioned: and, computing from the mean rate of the horary motion of the moon, it will be found that the duration of the occultation behind AB will be $20'.7''$ of time; and, behind DE, $18'.41''^*$. We therefore see how considerable is this difference of $1'.26''$ of time; and how well such observations are adapted not only to convince those who doubt the reality of the compression of the earth's axis, but likewise to show with very great approximation to truth the relative length of the earth's radii in different latitudes. For, in the case just mentioned, the variation of a twenty-thousandth part of that length † will cause a difference of one second of time in the duration of the occultation.

§ 11. Moreover, it is evident that the effect, of which I have

* For, joining C, A, and C, B, we shall have $CV=CA=CB=15'.45''$, and $CM=(15'.45''-60'')=14'.45''$; consequently $AM=MB=\sqrt{(AC-CM)}$. $(AC+CM)=5'.31'',4$; which, being multiplied by $\frac{60''}{32'.56'',5}$, in order to reduce this distance into time, will give $10'.3'',6$ for the time of the star's passing from A to M: therefore the time of the star's passing from A to B will be equal to $2 \times (10'.3'',6)=20'.7'',2$. But, if the depression of the earth's axis cause a variation in the apparent height of the moon equal to $8'',5$, then will $CL=14'.53'',5$, consequently (as $CD=AC$) $DL=LE=\sqrt{(AC-CL)}$. $(AC+CL)=5'.7'',7$; which, being also multiplied by $\frac{60''}{32'.56'',5}$, will give $9'.20'',5$ for the time of the star's passing from D to L: therefore the time of the star's passing from D to E will be equal to $2 \times (9'.20'',5)=18'.41''$. B.

† Or, about 1000 feet. The mean radius of the earth being 20,898,240 English feet. B.

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been speaking, may be much greater. It increases greatly as the line MV (or the ver-sine of the arc) becomes smaller: if, for example, it were $30''$, the duration of the occultation under AB would be $14'. 20''$; and, under DE, $12'. 10''$, being a difference, in the duration, of $2'. 10''$ of time*. In such a case, a difference of 500 feet in the length of the earth's radius might be rendered evident: since such a variation would produce a difference of one second of time in the duration of the occultation†.

§ 12. I am well aware that it scarcely ever happens that the apparent path of the star is rectilinear, and at the same time perpendicular to the vertical circle which passes through it, at the moment of the middle of the occultation, as in the case I have supposed. But such a supposition may be readily conceded, as being a mean between the possible cases. For, if the actual phenomena do in some cases deviate therefrom, so as to render the effect of the compression *less* upon the duration of the occultation than when calculated on the above hypothesis, it will on the other hand often occur that they are *greater*‡. Hence I have no hesitation in asserting that the means, which I have pointed out, are not dependent on any hypothesis, and are the most certain of any that have yet been suggested for determining the variation in the curvature of the earth's surface.

§ 13. With respect to the *pendulum*, the differences in its length are not only too minute and too difficult to be measured with precision, but are also too much influenced by the unknown internal density of the earth to be brought at all into comparison

* In this case, $CM = (15'. 45'' - 30'') = 15'. 15''$; consequently $AM = 3'. 56''. 2$: which being multiplied by $\frac{60'}{32'. 56''. 5} \times 2$, as in the preceding cases, will give $14'. 20''. 4$ for the time of the star's passing from A to B. But, supposing the depression the same as in the former instance, we should have $CL = 15'. 23''. 5$; consequently $DL = LE = 3'. 20''. 4$: which, multiplied also by $\frac{60'}{32'. 56''. 5} \times 2$, will give $12'. 10''. 2$ for the time of the star's passing from D to E, as mentioned in the text. B.

† According to my calculation, a difference of only 400 feet in the length of the earth's radius would produce a difference of one second of time in the duration of the occultation. But, if the line MV were only $15''$, a difference of less than 200 feet would produce a difference of one second of time in the duration. And, agreeably to these principles, it will be evident that the duration of such occultations will vary according to the elevation of the spot (from which they are observed) above the level of the sea; and will sensibly differ when observed in valleys, or on the tops of high hills and mountains. A fact which perhaps has not been sufficiently attended to, either in observations of occultations, or in calculations wherein the parallax of the moon is involved. B.

‡ I confess I do not see the accuracy of this conclusion: for it appears to me that the *effect* of the compression, on the duration of the occultation, must be *greatest* when the apparent path of the star is perpendicular to the vertical circle. B.

with

with the great differences, above mentioned, in the duration of occultations.

§ 14. The different *measurements of the degrees of the meridian* are likewise so discordant amongst themselves, that the respectable author (whom I am about to quote) asserts that the greater the number of degrees that are measured, the more uncertain does the figure of the earth seem to be*. Let us moreover bear

* This indeed appears to be the case from some of the late measurements. With respect to the meridian of France, M. Delambre states (*Traité d'Astronomie*, vol. iii. page 567) that "it was hoped that the measurement of that arc (divided nearly into two equal parts by the parallel of Evaux) would have given the quantity of the compression of the earth's axis. The calculation, however, makes it $= \frac{1}{30}$; a quantity much too great. The whole arc, compared with that of Bouguer at Peru, makes it $= \frac{1}{33\frac{1}{4}}$; which is too small." And he goes on to observe that, from a revision of the calculations of Lacondamine, and taking a mean between him and Bouguer, he has deduced the quantity $\frac{1}{30}$; which he considers as the most probable one, and which he proposes to be adopted in all future calculations. Nevertheless he afterwards states (*Ibid.* page 572) that a "compression of $\frac{1}{180}$ represents very well the arcs between Greenwich and Paris, between Dunkirk and Paris, between Greenwich and Evaux, between Greenwich and Barcelona, and between Carcassonne and Mount-Jouy. It appears then (he adds) that, without much variation, the arc of the meridian from Greenwich to Barcelona, indicates (in the whole and in its several parts) a compression which differs very little from $\frac{1}{180}$." And the conclusion, which he draws from the whole, is that "the arc of France indicates a compression more considerable than that of the globe in general." It is worthy of observation that Maupertuis, who was one of the astronomers that measured the arc of the meridian in Sweden in 1736, makes the compression $= \frac{1}{178}$, by comparing it with the arc then measured in France. But, M. Svanberg who, in 1805, remeasured the same arc, deduces the compression $= \frac{1}{307\frac{1}{4}}$ when compared with the arc recently measured in France; and $= \frac{1}{325\frac{1}{5}}$ when compared with that of Bouguer at Peru. This is however on the presumption that the standard measure, used in the survey, was equal to the double metre at zero of the thermometer: but, if it was so only at the temperature of $+16^{\circ}\frac{1}{4}$, the corresponding compressions would be $\frac{1}{326\frac{1}{6}}$, and $\frac{1}{331\frac{1}{4}}$. Other results are obtained according to the different tables of refraction made use of in the calculations; so that, after all, considerable doubts exist as to the exact quantity of the compression, as deduced from that survey. M. Delambre, from the measurements, given by Major Lambton, of an arc in the East Indies, stated the compression $= \frac{1}{205\frac{1}{4}}$; but, from the subsequent operations of the same gentleman, he afterwards deduces it $= \frac{1}{228}$. Major Lambton himself, however, from a more recent measurement has stated the compression to be $= \frac{1}{256}$, but, that on comparing the whole of his arc with the whole of the French arc, the compression would be $= \frac{1}{335}$. A remarkable discordance in the results. The measurement of Lacaille, at the Cape of Good Hope, compared with that at the equator, gives $\frac{1}{176}$. Lastly, I shall add that the recent measurement of the arc, in England, seems to indicate a *prolongation*, instead of a *compression* of the polar axis. These discordant conclusions leave a very unsatisfactory impression on the mind, notwithstanding the acknowledged

bear in mind the enormous expense and labour required in measuring a degree of the earth's surface. Let us recollect the great uncertainty that may attend such a measurement from the deviation of the plumb-line made use of in the instruments employed for the astronomical observations, occasioned by mountains and valleys, subterraneous cavities and other irregularities in the upper strata of the earth: and, of what great importance such anomalies may be, we may learn in the celebrated work of Boscovich*.

§ 15. It may perhaps be objected that the two conditions necessary for my purpose may not frequently occur together; namely, that the altitude of the moon †, and the chord of the lunar disc which passes over the star, must be small. This is probably the reason why this method of ascertaining the compression of the earth's axis has not hitherto been practised. It is true that the greater the altitude of the moon above the horizon, and the nearer to its centre the apparent path of the star, so much the smaller will be the difference of parallax caused by a sphere and an oblate spheroid ‡: and consequently the more easily might the

knowledge talents of the persons employed in those surveys; and prevent us from placing too great reliance on a method which produces results, differing so widely from each other. The learned Boscovich might well exclaim that "the greater the number of degrees that are measured, the more uncertain does the figure of the earth seem to be." B.

* *De Expeditione Litteraria, Lib. v. § 230, et seq.*

† It does not appear that the altitude of the moon should be limited to the quantity mentioned by the author in the text, since the variations, alluded to, will be perceptible at the height of 20, and even 30 degrees. Thus, in the case mentioned in § 9, it appears that the variation in the parallax of the moon at the height of 10° is 8'',5: but, if the moon were at the height of 20° above the horizon, the variation would be 8'',1; and at 30° it would be 7'',4. B.

‡ The smaller the chord of the lunar disc (or the smaller the ver-sine of the arc) the more perceptible will be the difference in the duration of the occultation in such cases; as will appear from the following table; which shows the duration of an occultation of the star on the supposition that the place of the star, when it passed the vertical circle, would be 60', 30', 15', 10', and 8'',5 within the moon's disc, provided the earth were a perfect sphere: together with the corresponding durations provided the earth were an oblate spheroid, having the axis compressed = $\frac{1}{300}$. Latitude of the place 60°: height of the moon 10°.

Earth = Sphere.		Earth = $\frac{1}{300}$.		Difference of Duration.
* within D's disc.	Duration.	* within D's disc.	Duration.	
60	20. 7	51,5	18.41	1.26
30	14.20	21,5	12.10	2.10
15	10.11	6,5	6.43	3.28
10	8.19	1,5	3.14	5. 5
8,5	7.41	0	Appulse.	7.41

the errors of the elements made use of in the calculation be confounded with that difference. But these are precisely the occasions most favourable for the observations with which these particular occultations may be compared; and by which (as I have said) the errors of the tables are corrected, and the geographical longitude determined.

§ 16. It is true that the conditions here required, for ascertaining the flattening of the poles, cannot be very frequently obtained: but, if we look out for them with the diligence that the importance of the object demands, we shall perhaps meet with them more frequently than we imagine. For, I may venture to assert that there does not pass a month without the occurrence of an occultation of some star whose position is well known: and there is no occultation that will not afford, to some part or other of the earth's surface, the conditions required*. If only once out of twenty times they should occur, in a place where there is an astronomical observatory, the question as to the compression, and also as to its quantity, would be very soon determined.

§ 17. For the solution of such questions, for which immense sacrifices have hitherto been made (for instance, in the measurement of the degrees of the meridian), it surely is not requiring too much that the trifling expense should be incurred of enabling astronomers to travel to places more favourably situated for making observations of such occultations. This would be an undertaking worthy of a sovereign who wishes to distinguish himself as a patron of science. We should, by such means, gradually arrive at a knowledge of the relative length of the terrestrial radii, in a great number of places: and, it is most probable that we might

In the APPENDIX I have given other tables, showing the differences that would arise from varying the latitude of the place, the height of the moon, and the quantity of the earth's compression: whereby the reader may be better able to judge of the maximum of difference which would arise under the most favourable circumstances. B.

* Since this was written, the positions of most of the zodiacal stars have been determined with a degree of accuracy sufficient for the purposes detailed in the memoir. The labours of *Cagnoli* himself, of *Piazzi*, *Harding*, *Zach*, and *Bessel* have contributed much to this end: so that we may now safely assert that scarcely a *night* passes "without the occurrence of an occultation of some star whose position is well known." Nearly forty years ago *Messier* made the following remark, at the close of a numerous list of observed occultations: "We see by this collection of occultations how many new ones I have observed, in the first quarter of the moon, on the dark limb; which are distinguished with the greatest precision. They are frequent, and much preferable to the observations of Jupiter's satellites, or lunar eclipses, for determining the longitude. It were much to be wished that the conductors of our Ephemerides should announce, for the first part of each lunation, the immersions of stars of even the 7th, 8th and 9th magnitude, which are as readily observed as those of the 1st, 2nd and 3rd magnitude." *Connaissance des Temps*, Année viii. page 319. B.

thereby be enabled finally to deduce the true and exact figure of the earth.

§ 18. Although it appears, from the different measurements of the degrees of the meridian, that the figure of the earth is not regular, still it is possible that the irregularities do not belong so much to the figure, or the radii, as to the nature of the upper strata, the different density of which may occasion the concealed error in the perpendicularity of the instruments: an error which (as I have elsewhere shown) may be quite sufficient to reconcile all the disagreements between the measurements hitherto taken*. Consequently the variations in the parallax in different latitudes might very well proceed with as much regularity as appears to exist in the variation of gravity, and in the length of the pendulum.

§ 19. It is in the power of every principal Academy materially to assist in such a discovery, by two methods. First, in regard to times *past*, to collect together, from all quarters, the observations of occultations stated to have been made in a given interval; for instance, in the last ten years: and to employ some calculator to select and compute all those which are proper for showing the variation of curvature at different places. Secondly, with respect to the *future*, to insert in the Ephemerides accurate notices of those places or districts where it would be most important that any occultation should be observed (particularly of the principal stars), in order that it might serve to apprise, and excite the attention of such astronomers as might be favourably situated themselves, or contiguous to more advantageous situations. It appears to me that such notices would be more important and useful than those of the phases of solar eclipses, about which the calculators of Ephemerides are in the habit of taking so much trouble.

END OF THE MEMOIR.

[The Appendix will be given in our next.]

* Independent of the deviations arising from the causes here alluded to, the plumb-line has been sometimes known to be attracted towards the sides of the *glass* vessel, containing the weight, as powerfully as gold-leaf towards an electrical tube. The remark appears to have been made by M. Flaugergues. "Astronomers ought to avoid using a *glass* vessel for the water in which the weight of the plumb-line is suspended; for, I have observed twice, in one year, a singular deviation in the plumb-line occasioned by the attraction of the ball of the plumb-line towards the side of the vessel in which it was suspended. This ball was drawn towards the side with as much rapidity as gold-leaf is attracted by an electrified tube: and I was obliged (in order to destroy the effect of this spontaneous electricity, so as to enable me to take equal altitudes) to put a coating of sealing-wax upon the ball. But, since I have substituted a *metal* vessel, this singular phenomenon has not again occurred." *Connaissance des Temps*, Année xiii. page 413. Although such a powerful impulse as this may not often have occurred, yet it is possible that slight deviations from the perpendicular may frequently have arisen from the cause here alluded to; and which may account for some anomalies which have been remarked in the observations made in the course of the surveys. B.

LXI. *Dissertation on Water Snakes, Sea Snakes, and Sea Serpents.* By C. S. RAFINESQUE, Esq.

WHENEVER a singular phænomenon, or an extraordinary natural occurrence, happens to be observed in the United States, whether spots in the sun, huge fossil bones, or serpents, a crowd of superficial writers hasten to offer us, instead of facts, their own ideas and conjectures on the subject, which prove, sometimes, more or less ingenious; but often wild, incorrect, or ridiculous. They are generally so much taken up by their own fancy, that they forget entirely to consult former writers of eminence on the same subjects, should they even happen to know of their existence. What idea are we to entertain of their attempts to explain those subjects, without availing themselves of the valuable writings of Herschel or La Place, Cuvier or Pinkerton, &c.? in whose works they had been previously and often completely illustrated. Let us listen to a group of children attempting to reason and argue on the rising of the sun, an eclipse of the moon, on the œconomy of bees, or on the structure of a whale, without asking any previous questions of their parents, and we shall find a great similarity between their thoughts and those of many of our speculative writers. They often contribute to render contemptible the subject of their inquiries, at least towards the vulgar, while it would otherwise become deeply interesting; and should their crude speculations ever reach Europe, they will certainly afford very unfavourable specimens of our knowledge and attainments in science. These reflections have naturally suggested themselves to my mind on the present occasion.

The ancients gave the name of Water Snakes and Sea Snakes to many fishes of the eel tribe, which bear an apparent likeness to land snakes, although they differ materially on examination, by having fins and gills, and neither lungs nor scales.

Many land snakes are in the habit of going into the water, in pursuit of their food, or to escape their enemies, and they have been called Water Snakes when found in that element.

Real Water and Sea Snakes had been noticed at a very early period by navigators in the Atlantic Ocean and the Indian Seas; but as they had not been destroyed, eminent naturalists had doubted their existence, believing that eels, or similar fishes, had been mistaken for snakes.

Russel was perhaps the first writer who established their existence beyond a doubt, by describing and figuring many of them, in his splendid work on the Snakes of the Coast of Coromandel. Schneider established for them his genus *Hydras*, which wrong name has been with much propriety changed into *Hydrophis*. They have since been described in all the works on enpetology,
by

by Shaw, Latreille, Daudin, &c. ; and those last writers have divided them into four genera, *Enhydria*, *Platurus*, *Pelamis*, and *Hydrophis* ; which form a peculiar tribe or natural family in the order of Snakes, to which I have given the name of *Platuria* (Platurians, Flat-tails or Water Snakes). They are completely distinguished from the land snakes, by having a compressed tail, which serves them as an oar or rudder, enabling them to swim with great swiftness ; and from the fishes of the eel tribe, by having neither gills nor fins. They breathe through lungs, at remote periods, whence they generally live near the surface of the water, like the animals of the whale tribe. They prey on fishes and sea animals, and some of them have venomous fangs. Many are known to come on land, like turtles, to deposit their eggs.

About fourteen species of Water Snakes have been described by the above authors ; ten more are noticed in the travels of Peron to Australia or New Holland, one of which was ten feet long ; and lately several monstrous species have been seen near our shores. Many others appear to have been perceived by former travellers ; and very probably a great variety are known to sailors. The knowledge of these animals is merely emerging into notice, and may be yet greatly improved. I shall not pretend to assert that they are as numerous as land snakes ; but it is very likely that one hundred species at least of this tribe exist in the waters of the ocean, lakes, and rivers. Intelligent travellers, seamen, and fishermen, will gradually make us acquainted with them : meantime, I shall endeavour to give a concise account of those we know, which may facilitate their future observations ; and I shall arrange my labour in a synoptical order, concluding by some remarks on the Sea Serpents, which are merely Sea Snakes of a very large size.

Family PLATURIA.—VI Genus. *Ophinectes*, Raf. Differing from *Pelamis* by having a compressed body and a carinated or angular abdomen. I arrange in this new genus all the Sea Snakes mentioned in Peron's Travels: they were all found on the western and southern shores of Australia, or New Holland ; such as may have fangs ought to belong to the genus *Natrix*, and those with cylindrical bodies to the genus *Pelamis*.

1. Sp. *Ophinectes cinereus*, Raf. Cinerous Ophinectes. Entirely gray or ash colour.

2. Sp. *Ophinectes viridis*, Raf. Green Ophinectes. Entirely green.

3. Sp. *Ophinectes luteus*, Raf. Yellow Ophinectes. Entirely yellow.

4. Sp. *Ophinectes cærulescens*, Raf. Blueish Ophinectes. Entirely of a blueish colour.

5. Sp. *Ophinectes versicolor*, Raf. Versicolor Ophinectes. Varied

Varied with many transverse cones, blue, white, red, green, and black. Many species are probably meant here.

6. Sp. *Ophinectes maculatus*, Raf. Spotted Ophinectes. Covered with many irregular large spots. Many species.

7. Sp. *Ophinectes punctatus*, Raf. Dotted Ophinectes. Covered with numberless small dots. Many species.

8. Sp. *Ophinectes erythrocephalus*, Raf. Red-headed Ophinectes. Head of a beautiful red; body * * * *.

9. Sp. *Ophinectes dorsalis*, Raf. Backed Ophinectes. Dark green with large spots of yellow and light green on the back; length three or four feet; near Dewitt's Land.

10. Sp. *Ophinectes major*, Raf. Large Ophinectes. Green spotted with red and brown. Length from eight to ten feet; also from the shores of Dewitt's Island.

This last species appears to be the largest real Sea Snake which has fallen under the personal observation of naturalists as yet. But larger species still have been noticed at different periods. If I had the time and opportunity of perusing all the accounts of travellers and historians, I could probably bring many into notice; but this tedious labour must be postponed, and I must warn those who may be inclined to inquire into the subject, not to be deceived by the imperfect and exaggerated accounts of ancient or unknown writers. Whenever they mention neither the scales nor tail of their Sea Serpents, or when they assert they had no scales, or had gills or fins, you must in all those instances be certain that they are real fishes rather than serpents. There might, however, be found some Sea Snakes without scales, since there are such land snakes; and there fishes with scales and yet without fins: but there are no fishes without gills, and no snakes or serpents with gills!—in that important character the classical distinction consists.

Nearly all the writers whom I can remember, have been unacquainted with that obvious distinction; and they have, in imitation of the ancient Greek and Roman writers, given the name of Sea Snakes to the large eels or fishes they happened to observe. This I apprehend is the case with Pontoppidan, in his Natural History of Norway; with Mongitore, in his Remarkable Objects of Sicily; with Leguat, in his Travels to Rodriguez Island, &c. Their observations, and the facts they record, are notwithstanding equally valuable, since they relate to monstrous unknown fishes, which seldom fall under the observation of men. The individuals of huge species are not numerous in nature, either on land or in water; and it is probable they often become extinct for want of food or reproduction.

Among the four different animals which have lately been observed

served by Americans, and named Sea Serpents, only one (the Massachusetts Serpent) appears to be such: another is evidently a fish, and two are doubtful. I shall offer a few remarks on each.

1. *The Massachusetts Sea Serpent.* From the various and contradictory accounts given of this monster by witnesses, the following description may be collected.—It is about 100 feet long; the body is round and nearly two feet in diameter, of a dark brown, and covered with large scales in transverse rows; its head is scaly, brown mixed with white, of the size of a horse's and nearly the shape of a dog's; the mouth is large, with teeth like a shark; its tail is compressed, obtuse, and shaped like an oar. This animal came in August last into the bay of Massachusetts, in pursuit of shoals of fishes, herrings, squids, &c. on which it feeds. Its motions are very quick: it was seen by a great many; but all attempts to catch it have failed, although 5000 dollars have been offered for its spoils. It is evidently a real Sea Snake, belonging probably to the genus *Pelamis*, and I propose to call it *Pelamis megophias*, which means Great Sea Snake Pelamis. It might however be a peculiar genus, which the long equal scales seem to indicate, and which a closer examination might have decided: in that case the name of *Megophias monstrosus* might have been appropriated to it.

2. *Capt. Brown's Sea Serpent.* This fish was observed by Capt. Brown in a voyage from America to St. Petersburg, in July 1818, near 60° N. latitude and 8° W. longitude, or north of Ireland. In swimming, the head, neck, and fore part of the body stood upright like a mast: it was surrounded by porpoises and fishes. It was smooth, without scales, and had eight gills under the neck; which decidedly evinces that it is not a snake, but a new genus of fish! belonging to the eighth order *Tremapnea*, 28th family *Ophictia*, and 3d sub-family *Catremia*, along with the genera *Sphagebranchus* and *Symbranchus* of Bloch, which differ by having only one or two round gills under the neck. I shall call this new genus *Octipos* (meaning eight gills beneath); head depressed, mouth transverse, large, eight transverse gills under the neck. And its specific name and definition will be *Octipos bicolor*. Dark brown above, muddy white beneath: head obtuse. Capt. Brown adds, that the head was two feet long, the mouth fifteen inches, and the eyes over the jaws similar to the horse's; the whole length might be 58 feet.

3. *The Scarlet Sea Serpent.* This was observed in the Atlantic Ocean, by the captain and crew of an American vessel from New-York, while reposing and coiled up, near the surface of the water, in the summer of 1816. It is very likely that it was a fish, and perhaps might belong to the same genus with the foregoing;
I shall

I shall refer it thereto, with doubt, and name it *Oclicpos? coccineus*. Entirely of a bright crimson: head acute. Nothing further descriptive was added in the gazettes where the account was given, except that its length was supposed to be about 40 feet.

4. *Lake Erie Serpent*. It appears that our large lakes have huge serpents or fishes, as well as the sea. On the 3d of July, 1817, one was seen in Lake Erie, three miles from land, by the crew of a schooner, which was 35 or 40 feet long, and one foot in diameter; its colour was a dark mahogany, nearly black. This account is very imperfect, and does not even notice if it had scales; therefore it must remain doubtful whether it was a snake or a fish. I am inclined to believe it was a fish, until otherwise convinced: it might be a gigantic species of eel, or a species of the above genus *Oclicpos*. Until seen again, and better described, it may be recorded under the name of *Anguilla gigas*, or Gigantic Eel.

ADDITIONS.

1. The *Pelamis megophias*, or Great Sea Snake, appears to have left the shores of Massachusetts, and to have baffled the attempts to catch it, probably because those attempts were conducted with very little judgement. But a smaller snake, or fish, nine feet long, and a strange shark, have been taken, of which the papers give no description: let us hope that they will be described by the naturalists of Boston.

2. It appears that another large species of Water Snake is noticed by D. Felix Azara, in his Travels in South America (Paris, 1809, 4 vols. 8vo), under the name of *Curiyu*, which may belong to the genus *Pelamis*, although this worthy traveller has omitted to describe its tail and scales. It may be called and characterized as follows: *Pelamis curis*. (*Curiyu*. Azara, Trav. vol. i. p. 226.) Spotted and variegated, of black and yellowish white. It measures over 10 feet, and is of the size of the leg: it lives in the lakes and rivers of Paraguay, north of the 31st degree of latitude. It goes sometimes on land (and among shrubs), but moves heavily: it has a dreadful aspect, but does not bite; it lives on fishes, young otters, apereas, and copibaras.

3. The Water Snake of Lake Erie has been seen again, and described to be of a copper colour, with bright eyes, and 60 feet long. It is added, that at a short distance balls had no effect on him: but it is omitted to mention whether it was owing to having hard scales (in which case it might be a real snake of the genus *Enhydriis* or *Pelamis*), or to the indexterity of the marksman.

4. Mr. W. Lee has brought to notice another Sea Snake, seen by him many years ago near Cape Breton and Newfoundland, which was over 200 feet long, with the back of a dark green: it stood

stood on the water in flexuous hillocks, and went through it with impetuous noise. This appears to be the largest on record, and might well be called *Pelamis monstrosus*; but if there are other species of equal size, it must be called then *Pelamis chloronotis*, or Green-back Pelamis.

5. Dr. Samuel Mitchill has exhibited to the Lyceum of Natural History, at the sitting of the 15th September, the specimen of a species of Sea Snake from his museum, sent him some years ago from Guadaloupe, by Mr. Ricord de Mariana, which appears to be another species belonging to the genus *Enhydris*, to which the name of *Enhydris annularis* may be given: we shall add its definition and description.

Enhydris annularis. Ringed Enhydris. Whitish, ringed with black, rings broader on the back, which is cinereous and rather angular in the middle; tail broad, short, obtuse, with 70 pairs of scales underneath; more than 200 pairs of abdominal scales. This animal is about 18 inches long, covered with smooth and roundish scales above: the head is depressed, obtuse, small, covered with similar scales, and nearly black; the lips are white; a white half ring sets on the nape of the neck, and extends on each side over the eyes; a black line connects the eyes with the nostrils; an oblong white band lies below the head, longitudinally; the nostrils are round; the mouth is small, and with a few small teeth; the body is cylindrical, but the back is slightly carinated towards its centre, and of an ash colour; the black rings are narrow underneath. The tail is only two inches long, very compressed; the extremity is broader, obtuse, tipped with white, and has a slight lateral angle on each side, or a protruding lateral nerve; a similar appearance is perceptible on the upper and lower edges, which appear to be thickened; the whole tail is covered with large scales of a transverse and broad shape. This snake is found in the West Indies, in the sea, particularly on the shores of the island of Guadaloupe.

6. A fabulous account of a great Water Snake, that, according to the Indian tradition, dwelt in ancient times in a lake near Philadelphia, may be seen in Dr. Barton's Medical and Physical Journal, vol. ii. p. 168. As another Indian tradition, relating to the mammoth, the megalona, &c. it may be partly founded on truth.

7. The great Sea Snake has been seen again towards the middle of September, in the bay of Massachusetts, and three yellow collars observed on his neck, which has led some to believe it might be another individual and species; but this circumstance might have been overlooked before. It is not stated whether it had streaks of a lighter hue on the body, as the first was represented

sented to have by some witnesses. It is therefore likely that the two characters of "streaks of a lighter hue on the body, and three yellow collars on the neck," may be added to its description. The collars are described as about two inches broad, and one foot apart.

S. Dr. Mitchill informs me that General Hawkins has written a memoir on the Sea Serpents of Massachusetts, which he has sent, with a drawing, to Sir Joseph Banks; it is a paper of some length, and much interest, as it relates facts, and all the circumstances attending the appearance and natural history of those huge animals, taken upon oaths of eye witnesses. He attempts to prove, with much probability, that several individuals have been seen, and two at least, if not three species; one with three collars, another without any, and a smaller one.

LXII. *Defence of English Periodical Mathematical Works, in Reply to Mr. MEIKLE.* By A CORRESPONDENT.

To Mr. Tilloch.

SIR, — IT has been said that the great Newton owed every thing to the native force of his genius, and scarcely any thing to his reading; and it would seem as though this were the case with your learned correspondent, Mr. Meikle. In your last Number he advances, with an air of novelty, three distinct properties of the ellipse, one of which has been long known, while the other two are the simplest possible deductions from equally well-known propositions; *well-known*, I mean, to a plain man like myself, who dare never venture upon announcing a proposition as *new*, until I have pretty carefully examined a few of the best treatises on the subject to which my supposed novelties belong.

Mr. Meikle first demonstrates that HG is less than CD . (I refer to the diagram at p. 291 of your last Number.) But this is an immediate deduction from a well-known property. Let CX be drawn, a semidiameter parallel to the tangent PK ; then it has been shown (see Robertson's *Conic Sections*, book ii. props. 19, 21) that $HG : CX :: CD : CB$. But CX , the consequent of the first ratio, is always less than CB , the consequent of the second: therefore HG , the antecedent of the first ratio, is less than CD , the antecedent of the second.

Mr. Meikle's second proposition, namely, that MI is greater than CB , is a simple deduction of the same kind; as he may find at his leisure.

With regard to the third property announced by Mr. M. viz. that $GH : IM :: DE^2 : AB^2$, it is certainly "very elegant;" but

but there are few propositions better known. It is given in the treatises on Conic Sections by Emerson, Hutton, Robertson, and Adams; in one or other of which Mr. Meikle would probably have found it, had it not been that he rather chooses to investigate for himself than to examine the investigations of others; a very good habit, doubtless, with certain restrictions, which I fear your correspondent has neglected.

Leaving, however, these matters to Mr. Meikle's consideration, my more serious objection is to the terms of depreciation which he applies to those periodical publications in this country which are principally devoted to mathematics. If Mr. Meikle did not "shut himself up in his own shell," or, which is nearly as unfavourable to the formation of habits of liberality, did not associate himself almost exclusively with some peculiar coterie, (I throw this out merely as a conjecture, knowing nothing of Mr. M. but from his papers in your valuable publication), he would know that the mathematical sciences in this country owe the most solid obligations to those periodical publications. He would know, that while the managers of some learned societies have for many years laboured hard to stifle mathematical knowledge, those publications, by presenting a strong and varied stimulus to young investigators, have done as much if not more than even Cambridge and Oxford to keep it alive:—he would know that some of the able philosophers from France, Germany, Denmark, and other countries, who have recently visited England, have so highly appreciated the value of *three* of those publications, viz. *The Ladies' Diary*, *The Gentleman's Diary*, and *Leybourn's Mathematical Repository*, as to take back with them complete series of each, that they might introduce into their own respective countries works formed upon the models of ours; or, as Mr. M. would say, might "torment *them* with such nonsense." Lastly, he would know, that many of the most eminent mathematicians of former days, such as Halley, Emerson, Simpson, Stewart, Landen, Wildbore, Lawson, Crakelt, Maskelyne, and among living mathematicians of established character, Bonycastle, Barlow, Brinkley (of Dublin), Dawson (of Sedberg), Gregory, Herschel, Hutton, Mudge, Robertson (of Oxford), Vince, and Wallace (of Edinburgh), commenced their mathematical career, and made considerable progress towards eminence, by contributing to those publications, or one or other of them.

Mr. Meikle affirms that those periodical publications are "mostly made up of *mere puzzling questions, totally useless and unconnected with science.*" To refute this assertion, I would entreat you to introduce at the end of this letter, the fifteen mathematical questions which appear in *The Ladies' Diary* for 1820, being the 117th number of that work, just issued from the press.

If Mr. Meikle will attempt the solution of some of those questions, especially the last four, he will be much better employed than in “assisting Sisyphus in rolling his stone.” This gentleman should also recollect that an inquiry which, at first sight, appears merely speculative, may ultimately be found of practical utility: of this we have instances in the well-known application of Diaphantine algebræ to the finding of fluents; and still more recently in the celebrated Gauss’s successful application of a function derived from arithmetical and geometrical progressions to some of the most intricate though interesting problems in physical astronomy. I am, sir,

Yours respectfully,

London, Nov. 16, 1819.

MATHEMATICUS.

Questions published in No. 117 of Ladies’ Diary for 1820.

1. What number is that which differs least from its common logarithm? and what number differs least from its hyperbolic logarithm?

2. Given the segments of the base made by the perpendicular from the vertical angle, to construct the plane triangle, when the *tangent* of one of the angles at the vertex made by the perpendicular has a given ratio to the *sine* of the other.

3. A cistern containing 5236 cubic inches, being filled with water, had dropped into it five heavy balls, whose diameters were in arithmetical progression. Required, the several diameters of the said balls; it being known that half the water was expelled in consequence of their immersion, and that the common difference of the terms of the progression was one inch.

4. Find two integers, such that their sum shall be a square, and their difference a cube number; but, if each of them be doubled, their sum shall be a cube, and difference a square number.

5. The three edges of a triangular pyramid which terminate in the vertex, are 12, 14, and 15; its perpendicular altitude 9; and the edges of its base are as the numbers 2, 4, and 5. Now, the distance of the centre of gravity from that angle of the base where the longest slant edge meets the two longest sides of the base being 6, what is the solidity of the pyramid?

6. Find the sides and areas, in whole numbers, of three scalene triangles, such, that their perimeters shall be equal, and their areas as the numbers 2, 7, and 15.

7. Give, by means of right lines and a circle, a general construction for the indeterminate equation $a^2 - 2a(x+y) + x^2 + y^2 + xy = 0$.

8. A person has to cross an elliptical common, the axes of which are eight and six miles, and has to call at a house situated in one

of the foci. On one side of the house he can walk at the rate of five miles an hour; but, on the other, only four miles an hour. Required the least time in which he can perform his rectilinear journey across the common.

9. Demonstrate, synthetically, that an arc of any curve cannot be of finite curvature, unless the subtense ultimately vary as the square of the arc.

10. In order that the eclipses of Jupiter's satellites may be visible at any place, the planet must be at least 8° above, and the sun 8° below, the horizon. Now, on the 1st of February 1821, there will be an eclipse of Jupiter's third satellite, the emersion taking place at $6^{\text{h}} 19^{\text{m}} 3^{\text{s}}$ P.M. Greenwich time: can that emersion be observed at Berlin, N. lat. $52^{\circ} 32' 30''$, E. lon. $13^{\circ} 26' 15''$? Sun's declin. S. $17^{\circ} 6'$. Jupiter's declin. S. $3^{\circ} 31'$. Passage merid. $2^{\text{h}} 39^{\text{m}}$ P.M.

11. Suppose BC to be perpendicular to the horizontal line AC, and both of them to be given in length, and that a string of a given length (greater than $\sqrt{AC^2 + BC^2}$) is fastened at its two extremities to the points or tacks A, B; and that the said string, as it is moved round A, is stretched tight by the continual motion of a point along the horizontal plane. Required, the curve that will thus be described on the horizontal plane, by the said stretching or tending point.

12. What will be the ratio of the forces of gravity, at the surface of the earth, at the top of a slender cylinder a mile high, at the upper surface of a sphere a mile in diameter (in contact with the earth and of the same medium density), and on the top of an extensive piece of table-land of the same density and a mile high, taking the earth's radius at 3960 miles?

13. In the great solar eclipse which happens Sept. 7th, 1820, the apparent time of the greatest obscuration at Greenwich is $1^{\text{h}} 52^{\text{m}} 48^{\text{s}}.2$ P.M. when the angular distance about the centre of the sun from its vertex to the centre of the moon is $17^{\circ} 18' 22''$ to the left hand; and the visible distance of their centres to the difference of their horizontal parallaxes as 0.5665946 to 1. Hence it is required to find the direction and distance from the above place, which a person must travel for the shortest *journey* to observe the sun centrally eclipsed; and at the same time the angle of elevation, and the direction he must ascend in a balloon for the shortest *voyage* to be gratified with a sight of the same phenomenon; supposing the earth a perfect sphere, and its diameter 7914 miles?

14. It is required to investigate a theorem, comprehending the pressure on the base of a steam-engine piston, the course or stroke of that piston and its velocity, on one part; and the velocity, mass,

mass, and diameter of a fly, on the other part; so that one rotation of the fly, with its initial velocity, shall produce a dynamic effect equal to that of the piston in n successive strokes.

N. B. The mass of the fly is supposed equally distributed over its rim, and the diameter of the crank handle equal to the course of the piston.

15. The major and minor axes of an elliptical billiard-table are $2a$ and $2b$. Suppose an elastic ball to be propelled through one of the foci perpendicularly to the major axis, what will be the rectangular co-ordinates which indicate its position at the tenth reflection? and will it, after any finite number of reflections, move to and fro in the direction of the major axis?

LXIII. *On the Figure of the Earth.* By M. DE LAPLACE*.

IT has been proved by numerous experiments made with the pendulum, that the increase of gravity follows a very regular progression, and is nearly as the square of the sine of the latitude. This force being the result of attractions of all the terrestrial molecules, observations thereon, compared with the theory of the attraction of spheroids, offer the only means that can enable us to penetrate into the internal constitution of the earth; and the result is, that the earth is formed of strata, of which the density increases from the surface to the centre, round which they are regularly arranged. In the *Connoissance des Temps* for 1821, I published the following theorem, which I demonstrated in vol. ii. of the *Nouveaux Mémoires de l'Académie des Sciences*.

“If we take the length of the seconds pendulum at the equator as unity, and if to the length of this pendulum, observed at any point on the surface of the terrestrial spheroid, be added, half the height of this point above the level of the sea, divided by half the polar axis, a height which is given by barometrical observation, the increase of this length, thus corrected, will be, on the hypothesis of a constant density below a small depth, equal to the product of the square of the sine of the latitude by five-fourths of the ratio of the centrifugal force to the gravity at the equator, or by 43 ten-thousandths.”

The above theorem is generally true, whatever may be the density of the sea, and the manner in which it covers the earth. Experiments made with the pendulum in both hemispheres agree in giving to the square of the sine of latitude, a coefficient somewhat larger—nearly equal to 54 ten-thousandths. These experiments, therefore, prove sufficiently that the earth is not homogeneous in

* From *Annales de Chimie et Phys.* tome xi.

the interior, and that the density of the strata increases from the surface to the centre.

But though the earth be, in a mathematical sense, heterogeneous, it may notwithstanding be chemically homogeneous, if the increase of density of its strata is caused only by the increased pressure they suffer as they approach the centre. It may easily be conceived that the immense weight of the superior strata may considerably increase their density, though they may not be fluid; for it is known that solid bodies are compressed by their own weight. The law of the densities which result from these compressions being unknown, we cannot tell how far the density of the terrestrial strata may be thus increased. The pressure and the heat which we can produce are very small, compared to those which exist at the surface, and in the interior of the sun and stars. We cannot even form an idea of the effect of these forces, united in those immense bodies. Every thing tends to make us believe that they existed at one time in a high degree on the earth, and that the phænomena which they have occasioned, modified by their successive diminution, form the present state of the surface of our globe; a state which is nothing more than the element of a curve, of which time is the abscissa, and of which the ordinates will represent the changes that this surface has suffered without ceasing. We are far from knowing the nature of this curve, and we cannot therefore ascend with certainty to the origin of what we observe on the earth; and if, to satisfy the imagination, always troubled by ignorance of the cause of the phænomena which interest us, a few conjectures are hazarded, they should be offered with the utmost caution.

The density of a gas, the temperature remaining the same, is proportional to its compression. But this law, though true within those limits of density in which we have been able to prove it, cannot be applied to liquids and solids, of which the density is very great, compared to that of gas, when the pressure is little or nothing. It may naturally be supposed that these bodies resist compression the more they are compressed; so that the ratio of the differential of the pressure to that of the density, instead of being constant, as with gases, increases with the density. The most simple function which can represent the ratio, is the first power of the density, multiplied by a constant quantity: and this I have adopted, because it unites to the advantage of representing in the simplest manner what we know of the compression of liquids and solids, a facility of calculation in researches on the figure of the earth. Hitherto, mathematicians have not included in this research the effect resulting from the compression of the strata. Dr. Young has called their attention to this object, by the ingenious remark, which may be thus stated, the increase of
density

density of the strata of the terrestrial spheroid. I have supposed that some interest may be excited by the following analysis, from which it appears that it is possible to explain all the known phænomena depending on the law of the density of these strata. These phænomena are the variation of the degrees of the meridian, and of gravity, the precession of the equinoxes, the nutation of the terrestrial axis, the inequalities which the flattening of the earth produces in the motion of the moon, and lastly, the ratio of the mean density of the earth to that of water, which Cavendish has fixed by an admirable experiment at five and a half. In proceeding from the law already announced of the compression of liquids and solids, I find that, if the earth be supposed to be formed of a substance chemically homogeneous, of which the density is $2\frac{1}{4}$ that of common water, and which compressed by a vertical column of its own substance, equal to the millionth part of half the polar axis, will augment in density 5.5345 millionths of its first density, it will account for all the phænomena. The existence of such a body may be easily admitted, and is apparent from the surface of the earth.

If our globe were entirely formed of water; and if, in conformity with Canton's experiments, it be supposed that the density of water at 10° (50° Fahr.) and compressed by a column of water 10 metres (32.81925 ft.) in height increases by 44 millionths, the flattening of the earth would be $\frac{1}{1060}$; the coefficient of the square of the sine of the latitude in the expression of the length of the seconds pendulum would be 59 ten-thousandths, and the mean density of the earth would be nine times that of water. These results differ from observations by more than the errors to which they are liable.

I have been supposing the temperature uniform throughout the whole extent of the terrestrial spheroid; but it is very possible that the heat is greater towards the centre, and that would be the case if the earth, originally highly heated, were continually cooling. The ignorance in which we are with respect to the internal constitution of this planet, prevents us from calculating the law by which the heat decreases, and the resulting diminution in the mean temperature of climates; but we can prove this diminution to have been insensible for these 2,000 years.

Let us suppose a space of a constant temperature, containing a sphere possessing a rotary motion; and that, after a long time the temperature of the space diminishes one degree; the sphere will finally take this new temperature; its mass will not be at all altered, but its dimensions will diminish by a quantity which I will suppose to be a hundred thousandth, a diminution which is nearly that of glass. In consequence of the principle of areas, the sum of the areas which each molecule of the sphere will de-

scribe round its axis of rotation will be the same in a given time, as before. It is easy to conclude from this, that the angular velocity of rotation will be augmented by a fifty thousandth. So that, supposing the time of a rotation to be one day, or a hundred thousand decimal seconds, it will be diminished two seconds by the diminution of a degree in the temperature of the space. If we extend this consequence to the earth, and also consider that the duration of the day has not varied since the time of Hipparchus, by the hundredth of a second, as I have shown by the comparison of observations with the theory of the secular equation of the moon, we shall conclude, that since that time, the variation of the internal heat of the earth is insensible. It is true that the dilatation, the specific heat, the degree of permeability by heat, and the density of the various strata of the earth being unknown, may cause a sensible difference between the results relative to the earth, and those of the sphere we have supposed; according to which the diminution of the hundredth of a second, in the length of the day, would correspond to a diminution of two hundredths of a degree of temperature. But this difference could never extend from two hundredths of a degree, to the tenth; the loss of terrestrial heat corresponding to the diminution of a hundredth of a second in the length of the day. We may observe, even that the diminution of the hundredth of a degree, near the surface, supposes a much greater one in the internal strata; for it is known that ultimately the temperature of all the strata diminishes in the same geometric progression, so that the diminution of a degree near the surface, corresponds to a much greater diminution in the strata nearer to the centre. The dimensions of the earth, therefore, and its *inertial momentum* would diminish more than in the case of the sphere we have supposed. Hence it follows, that if, in the course of time, changes are observed in the mean height of the thermometer placed at the bottom of the observatory caves, it must be attributed not to a variation in the mean temperature of the earth, but to change in the climate of Paris, of which the temperature may vary, with many accidental causes. It is remarkable that the discovery of the true cause of the secular equation of the moon, should at the same time make known to us the invariability of the length of the day, and of the mean temperature of the earth since the time of the most ancient observations.

This phenomenon induces a belief that the earth has arrived at that permanent temperature, which accords with its position in space, and its relation to the sun. It is found by analysis, that whatever be the specific heat, the permeability by heat, and the density of the strata of the terrestrial spheroid, the increase of the heat, at a depth very small, compared to the radius of that spheroid,

roid, is equal to the product of that depth, by the elevation of the temperature of the surface of the earth, above the state of which I have just spoken, and by a factor independent of the dimensions of the earth, and which depends only on the qualities of its first stratum relative to heat. From what we know of these qualities we find that if this elevation was many degrees, the increase of heat would be very sensible at depths to which we have penetrated, and where nevertheless it has not been found.

Note added by the Editor of Annales de Chimie, &c.

Our readers, we think, will not be displeased to find here some details of the method by which M. de Laplace has established the invariability of the duration of the day:—

A mean solar day is equal to the time occupied by one revolution of the earth on its own axis, increased by the mean apparent motion of the sun, in the same interval. Theory has proved that the mean apparent motion of the sun, like that of all the planets, is constant; the duration of a solar day, therefore, can only vary by a change in the velocity of the rotation of the earth.

The time in which the moon returns to the same position, relative to the sun, for example, its conjunction, is called a lunar month. This interval is evidently independent of the velocity of the earth's rotation. Our globe might even cease to turn on its centre, without the moon's advancement in its orbit suffering any alteration. From hence results a very simple method of discovering if the duration of the solar day has changed.

Suppose the duration of a lunar month to be now ascertained by direct observation; that is, how many days, and fractions of days, the moon occupies in returning to its conjunction with the sun. It is evident that on repeating this observation at another time, a different result will be found, if the length of the day has changed, if at the same time the velocity of the moon has not changed. The month will appear longer, if the length of the day has diminished; and on the contrary, shorter, if the day has increased. The constancy of the lunar month will indicate the invariability of the length of the day.

All observations combine to prove that from the time of the Chaldeans, to our own days, the duration of the lunar month has been gradually diminishing. It follows, therefore, from what has been stated, either that the velocity of the moon has increased, or that the solar day has lengthened. But M. de Laplace has discovered by theory, that there is in the motion of the moon, an inequality known by the name of *secular equation*, which depends on the variation of the excentricity of the earth's orbit, and of which the value in each century may be deduced from the change of this excentricity. By the assistance of this equa-

tion, the increase of the forementioned velocity is perfectly accounted for. There is, therefore, no reason to suppose that the duration of the day is not sensibly constant.

Let us for a moment admit, with M. de Laplace, that this duration surpasses at present that of the time of Hipparchus, by the hundredth of a decimal second. The duration of a century now, or of 36,525 solar days, would be longer than the duration of a century 2,000 years ago, (Hipparchus lived about 120 years before our æra,) by 365."25. In this interval of time, the moon describes an arc of 534".6; this quantity, therefore, expresses the difference between two arcs traversed by the moon in a century now, and in one of the time of Hipparchus; but as these arcs, determined by observation and corrected by the secular equation, do not differ by a quantity so large, we may conclude that in this long interval the duration of the day has not varied one hundredth of a second.

LXIV. *Account of some remarkable Facts observed in the Deoxidation of Metals, particularly Silver and Copper. By SAMUEL LUCAS, Esq.**

Sheffield, May 31, 1815.

DEAR SIR,—**W**HEN I had the pleasure of seeing you in Manchester, I mentioned having observed that pure silver, when melted, and while in a fluid state, had the property of uniting with a small proportion of oxygen, not only from the atmosphere, but also from other bodies which gave it out at a suitable degree of heat, as some of the nitrates for instance; and that the oxygen thus absorbed remains united with the silver only so long as it continues in a fluid state, or, while fluid, until some substance be applied having a more powerful attraction for the oxygen. In proof of this, I now send, for your inspection, a few specimens of silver that has been in the different states, and which carry the external marks; and also a bottle of the gas collected from silver, which had been exposed to the influence of the atmosphere by cupellation.

If silver in large quantities, after having been exposed in a melted state to a current of oxygen gas or atmospheric air, be allowed gradually to cool, the surface first becomes fixed or solid; this soon bursts, ebullition ensues, and an elastic vapour in considerable quantity escapes, driving before it a portion of the internal fluid metal, which, becoming solid as it is brought to the surface, produces the protuberances as shown by the accompanying specimen No. 1. This ebullition continues from $\frac{1}{4}$ to $\frac{1}{2}$

* From Manchester Memoirs, vol. iii.

an hour or more, according to the quantity of silver, and the rapidity with which it is cooled.

If, instead of cooling gradually, it be made to assume the solid state suddenly by pouring it into water, still the same phenomena occur; an ebullition takes place, and oxygen gas is evolved; but as the silver is so much divided, and passes so suddenly from the fluid to a solid state, the protuberances are proportionably minute, and are spread more equally over the whole surface, as will be seen in specimen No. 2.

No. 3 shows the arrangement of crystallization, which the silver assumes when the gas is separated from it, during the time of its becoming solid.

I have before observed, that substances having a powerful affinity for oxygen, will take it from the silver, even while in a fluid state. Thus, if charcoal be spread, for a few moments only, on the surface of silver that has absorbed oxygen, the whole of the oxygen will immediately be taken from it; no ebullition or escape of gas occurs, whether it be cooled gradually, as in specimen No. 4, or when poured into water, as in No. 5. By comparing these two specimens with Nos. 1 and 2, a very great difference will be observed, which is occasioned wholly by the escape of gas from the latter, while no such circumstance attended the former.

The bottle of gas which you will receive herewith, was collected in the following manner. Some silver, after cupellation, till in a state of perfect purity, was poured, by a few pounds at a time, into a vessel containing about 30 gallons of water; and an inverted bottle previously filled with the water, and with a funnel in its mouth, being instantly placed over the silver, as it was each time poured into the water, the gas, as it was given out and arose from the silver, was thus collected in the bottle until it was filled.

Care is necessary, that the neck of the bottle be kept below the surface of the water to prevent the access of atmospheric air, and I am not very certain that there is not a little admixture*.

In addition to the above, I have inclosed two samples of copper, in two different states, both, however, equally pure, except that the one is believed to be combined with oxygen, and the other not.

No. 1, is a sample taken from a furnace-full of about 5 cwt., when in a melted state, and which had been exposed uncovered to a current of atmospheric air for about two hours before and during the time it was melting. This when poured into water exploded most violently, as will be seen by the small, which was attempted to be granulated.

* I found this gas to contain 86 or 87 per cent. of oxygen. J. D.

The specimen No. 2, is a sample from the same copper, after the surface had been covered with charcoal for about half an hour. This, you will perceive, is in a very different state from the other, and, when poured into water, granulated without any explosion, as the small bits will show. I remain, &c.

To Mr. John Dalton.

SAMUEL LUCAS.

LXV. *Singular Anecdote of the Spider, with Observations on the Utility of Ants in destroying venomous Insects. By Captain BAGNOLD**.

DESIROUS of ascertaining the natural food of the scorpion, I inclosed one (which measured three quarters of an inch from the head to the insertion of the tail) in a wide-mouthed phial, together with one of those large spiders so common in the West Indies, and closed it with a cork perforated by a quill for the admission of air: the insects seemed carefully to avoid each other, retiring to opposite ends of the bottle, which was placed horizontally. By giving it a gradual inclination, the scorpion was forced into contact with the spider, when a sharp encounter took place, the latter receiving repeated stings from his venomous adversary, apparently without the least injury, and, with his web, soon lashed the scorpion's tail to his back, subsequently securing his legs and claws with the same material. In this state I left them some time, in order to observe what effect would be produced on the spider by the wounds he had received. On my return, however, I was disappointed, the ants having entered and destroyed them both.

In the West Indies I have daily witnessed crowds of these little insects destroying the spider or cockroach; as soon as he is dispatched, they carry him to their nest. I have frequently seen them drag their prey perpendicularly up the wall; and although the weight would overcome their united efforts, and fall to the ground perhaps twenty times in succession, yet, by unremitting perseverance, and the aid of reinforcement, they always succeeded.

A struggle of this description once amused the officers of His Majesty's ship *Retribution* for nearly half an hour: a large centipede entered the gun-room, surrounded by an immense concourse of ants; the deck for four or five feet round was covered with them, his body and limbs were encrusted with his lilliputian enemies, and although thousands were destroyed by his exertions to escape, they ultimately carried him in triumph to their dwelling.

In the woods near Sierra Leone I have several times seen the

* From the Quarterly Journal, No. xv.

entire skeletons of the snake beautifully dissected by these minute anatomists.

From these circumstances it would appear, that ants are a considerable check to the increase of those venomous reptiles, so troublesome in the torrid zone: their industry, perseverance, courage, and numerical force, seem to strengthen the conjecture: in that case they amply remunerate us for their own depredations.

LXVI. *Notices respecting New Books.*

DR. PINCKARD has just published a small work, containing four Cases of violent Death following the Infliction of Wounds made by the Bite of Dogs. He considered the diseases which followed as Hydrophobia, but the symptoms in all, according as they are related in the work, varied: in the first case, the greatest aversion to water prevailed; in the second, the patient drank freely of that element by his own desire. But the most unsatisfactory circumstance, in all the four cases, is that there is no proof of the dogs themselves being mad at all:—three of them ran away and were never heard of, and the fourth was almost immediately killed. The impolicy of destroying dogs in such cases, instead of tying them up to see if they turned out mad, is the most obvious fact illustrated by the publication in question. It is well known that punctured wounds in certain constitutions have produced *tetanus* and other sort of convulsions ending in death, without the concurrence or aid of any specific virus; and we must still lament that no satisfactory account has ever been given of the mysterious disease called Hydrophobia.

The concluding part of Dr. Rees's *Cyclopædia* is about to make its appearance. This elaborate and highly useful work has been conducted with a degree of spirit and liberality highly creditable to the Editor and Publishers; and, which is not always the case with such extensive undertakings, improving from its commencement to its termination. To the spirited proprietors are the public indebted for setting an example which none will venture henceforth to disregard in works of this nature, by giving the illustrative plates in a style of drawing and engraving which reflects great credit on the contrivers and executors of this great national work. We would suggest that at least one supplementary volume should be added to the work, for the purpose of giving such additional discoveries and improvements in the sciences and arts as have been made at various times since the work was commenced, and
for

for which suitable places could not be found in the then unpublished volumes; that the work may be in every respect complete. The subscribers we are sure would receive this as a favour.

Preparing for Publication.

Paris: consisting of 60 Engravings by Mr. Charles Heath, and other artists, from views taken in Paris, and its vicinity, by Capt. Batty, of the First or Grenadier Guards. The work will be published in twelve numbers, each containing five plates.

Views in Paris and its Environs, in a Series of from 50 to 60 Engravings from Drawings by Mr. Frederic Nash. The work, accompanied with appropriate letter-press, will be published in ten parts.

Gideon Mantell, Esq. has in the press a work on the Fossils of the South Downs, with Outlines of the Mineral Geography of the environs of Lewis and Brighthelmston, in a quarto volume, with Engravings.

The Rev. Mr. Ward, of Serampore, Bengal, has in the press the 3d and 4th Volumes of his View of the History, Literature, and Religion of the Hindoos; including a minute Description of their Manners and Customs, and Translations from their principal Works.

The Third Volume of Messrs. Kirby and Spence's Introduction to Entomology is in considerable forwardness.

"A Journey in Carniola and Italy," by W. Cadell, Esq. two vols. 8vo. with Engravings, is ready for publication.

Mr. Crawford's History of the Indian Archipelago, in three volumes, 8vo. with Engravings, is also nearly ready.

Mr. Scoresby's Account of the Arctic Regions will soon appear. It includes the Natural History of Spitsbergen and the adjacent Islands, the Polar Ice, and the Greenland Seas; with a History, &c. of the Northern Whale Fishery; chiefly derived from Researches made during seventeen Voyages to the Polar Seas: two volumes, 8vo., with Engravings.

Williams's Travels in Italy, Greece, and the Ionian Islands, embracing Manners, Scenery, and the Fine Arts, with Engravings, two volumes, 8vo., will also speedily appear.

Mr. J. P. Arrowsmith is printing "The Art of Instructing the Infant Deaf and Dumb;" with Copper-plates drawn and engraved by the Author's brother, who was born deaf and dumb.

A Series

A Series of Portraits of the British Poets from Chaucer to Cowper and Beattie is now in considerable forwardness. They are to be engraved in the line manner by the first artists, from authentic originals. It is to be published in 25 parts, each containing six engravings, and will form two volumes.

Professor Jameson, who has for many years been investigating the mineralogical structure of Scotland, will soon present to the public a Map of the mineralogy of that country.

Mons. Delaunay, late Superintendant of the Manufactories of the Prince of Monaco, encouraged by the recommendation of MM. Chaptal, Berthollet, Darcet, and other principal chemists of France, has translated the valuable Chemical Essays of Mr. Parkes into the French language. The first volume has been announced for publication at Paris this month.

LXVII. *Proceedings of Learned Societies.*

ROYAL COLLEGE OF SURGEONS, LONDON.

ONE of the Jacksonian prizes for the year 1818 not having been awarded, the College has proposed two questions for 1820, viz. "Diseases of the Skin," and "Diseases of the Rectum."

The candidates to be members of the College, not on the Court of Assistants. Dissertations to be in English, and the number and importance of facts will be considered principal points of excellence. Each dissertation to be distinguished by a motto or device, and accompanied by a paper sealed up containing the name and address of the author, and having on the outside a motto or device corresponding with that on the dissertation. No dissertation, nor motto or device, to be in the hand-writing of the author, nor sealed paper to have the impression of his seal. Dissertations to be delivered to the Secretary, at the College, before Christmas-day 1820. Those that are unapproved of, will be returned on authenticated application.

The prize subject for the present year is, *the Treatment of morbid local Affections of Nerves*. To promote the knowledge of which, it is required that a minute dissection of the nerves of the cervical portion of the Medulla spinalis, and of their communications with other nerves, be made; and it is expected that such dissections be authenticated by preparations of the dissected parts.

Dissertations to be delivered at the College before Christmas-day next.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Sixth Annual Report of the Council.

The state of comparative maturity to which the Society has now arrived, affords less interesting matter for remark than during its early progress: The Council, therefore, in discharging this their annual duty to the members, have little left them to do, but to call their attention to the respectable rank which the Institution has attained, and to urge the necessity of their continued patronage to ensure its stability.

Independently of the intrinsic advantages of an institution of this kind in gradually adding, by the labours of its members, to the knowledge of the physical structure of Cornwall, it possesses a secondary value by attracting to this part of the county individuals eminent for their genius and scientific acquirements, whose presence cannot fail to be useful to any place which they visit.

Owing to expenses incidental to the completion of a new Museum, the funds of the Society have not, as was expected, as yet justified the addition, by purchase, of any new minerals to the Cabinet: neither have the donations been so numerous and splendid as last year. The Society has, however, been favoured with not a few specimens, as well from members as others.

The communications on Geology and the branches of science connected with it, have been numerous and valuable, and the quantity of information contained in several of these respecting the structure of the county and its mineral repositories, renders it the duty of the Council to lay them before the public as soon as materials for a second volume are accumulated,—a period probably at no great distance.

The Council regret that the backwardness of many of the members who have it most in their power to forward some of the most interesting objects of the institution, justifies, and indeed renders necessary, the repetition of the following appeal to their liberality and zeal:

“The Council cannot avoid expressing their regret that so few new specimens have been obtained from the county mines; and that, consequently, the department of the Cabinet set apart for the reception of indigenous ores, which ought to be particularly rich and splendid, continues to be defective, and is eclipsed by many other collections, as well public as private;—a circumstance uniformly exciting the surprise of strangers.

“The Council earnestly request the attention of members to the grand object of the institution, that, namely, of enlarging our knowledge of the Geological structure of Cornwall. It is impossible for a few members to undertake the investigation of the whole county.—It is therefore hoped, that, with a view of enabling

abling the Society to complete its long-promised but still very defective Geological Map, members will, in their respective districts, endeavour to ascertain the nature and relations of the rocks, and transmit their observations made, and specimens collected, from time to time, to the Secretary, who will be very ready to assist their inquiries by any advice or information in his power. Any person, even although unacquainted with the principles of Geological science, can, it is obvious, collect specimens of the various rocks in his vicinity; and members are requested to bear this in mind, with the assurance that collections of this kind, with the various *localities* of the specimens affixed, will very materially promote the important object in view. One grand *desideratum*, and which might be very easily supplied by members resident in the different parts of the county,—is, to ascertain the exact *limits of the different granite and killas districts*.—The farmers and miners, in any part of Cornwall, could give this information to any gentleman that would take the trouble to record it, or to trace the boundary lines in any of the county maps.”

By order,

Sept. 21, 1819.

JOHN FORBES, Secretary.

The following Papers have been read since the last Report.

I. On the Throw of Veins. By Frederic Hall, Esq.—II. On the Importance of Mineralogical and Geological Knowledge to the practical Miner. By John Forbes, M.D. Secretary.—III. On the Granite Veins of Cornwall. By Joseph Carne, Esq. F.R.S. Hon. M.G.S. Member of the Society.—IV. An Account of the Alluvial Depositions at Sandrycock. By the late P. Rashleigh, Esq.—V. Observations on the Alluvial Strata of Poth, Sandrycock, and Pentuan. By John Hawkins, Esq. F.R.S. M.G.S. Honorary Member of the Society.—VI. On the Precipitation of Copper. By Joseph Carne, Esq. F.R.S., &c.—VII. On the Geology of Saint Michael's Mount. By Dr. Forbes.—VIII. On Elvan Courses. By Davies Gilbert, Esq. Vice-President of the Royal Society, President.—IX. On the Intersection of Lodes in the Direction of their Dip or Underlie. By John Hawkins, Esq. F.R.S., &c.—X. On the Geology of the West of Cornwall; Part second. By Dr. Forbes.—XI. Appendix to the above. By Professor Jameson.—XII. Observations and Experiments on the Construction and Use of a Safety Bar. By John Ayrton Paris, M.D. F.L.S. Honorary Member of the Society.—XIII. On the different Processes employed in Blasting Rocks; being an Appendix to Dr. Paris's Paper. By Dr. Forbes.—XIV. On the Temperature of the Mines of Cornwall. By R. W. Fox, Esq. Member of the Society.—XV. On the Temperature of Mines. By Dr. Forbes.—XVI. Notice on the Geology of the Neighbourhood of

of Sidmouth. By C. Worthington, Esq.—XVII. On the Origin of the Cornish. By the Rev. Samuel Greatheed.—XVIII. Notice on the Cornish Minerals in the British Museum.—By C. Konig, Esq. F.R.S. Honorary Member of the Society.—XIX. On the Transmission of Heat through different Surfaces. By R. W. Fox, Esq.—XX. Notice on the Coal Field of Pontypool. By W. Llewellyn, Esq.—XXI. On an Ebbing and Flowing Spring. By Monsieur Tracelle.—XXII. Notice on the Employment of a Mixture of Sawdust and Gunpowder in Blasting Rocks. By Sir Christopher Hawkins, Bart. M.P. F.R.S. a Vice-President of the Society.—XXIII. Notice of the Quantity of Tin and Copper raised in Cornwall; and of the Quantity of Copper raised in Great Britain and Ireland in the year ending 30th June 1819. By Joseph Carne, Esq. F.R.S., &c.

BATH INSTITUTION.

It is proposed to form in Bath, an Institution for the cultivation of Science, Literature, and the liberal Arts.

“The Institution to consist of a house and establishment, comprising the following accommodations: namely, a library and reading-room, from which newspapers and political pamphlets shall be excluded; a botanic garden; a museum of natural history; a cabinet of mineralogy; a cabinet of antiquities; a cabinet of coins and medals; a hall for lectures, with suitable apparatus for the courses on chemistry and the several branches of natural philosophy.

“To these will be added an exhibition gallery, for the reception and display of paintings and other works of the fine arts.

“The funds to be raised by subscriptions for shares of 50*l.* each, and the right of property to be vested in the subscribers.

“The incorporation of the subscribers to take place under a Legislative Charter.

“The management of the Institution to be conducted by a Board of Directors.

“The Institution to be open to annual and life subscribers.

“A capital sum of thirty thousand pounds will be required for carrying the general purposes into effect. Twenty thousand pounds to be disposable in the purchase of premises, erecting the necessary buildings, and fitting up the Institution in a suitable manner; and ten thousand pounds to form a reserved fund, the interest of which shall be applicable to defraying the annual expenses.

“No active proceedings to be commenced, until there shall be subscriptions for at least 300 shares.

“The Provisional Constitution of the intended Establishment may be inspected at the ‘Treasurers’, Messrs. Cavenagh and Co.,
by

by those who may wish for more full and precise information previously to subscribing.

“The amount of subscription will be taken by instalments of sums not exceeding five pounds, and at intervals not shorter than three months.”

THE EGYPTIAN SOCIETY.

The object which this Society has in view, and which is certainly an important one, will be understood from the following *Prospectus of a work to be entitled, Hieroglyphics collected by the Egyptian Society*.—The triple inscription of Rosetta having afforded a prospect of the partial interpretation of the Egyptian hieroglyphics in general, it becomes a matter of high importance, for the advancement of literature and of the study of antiquities, to collect and preserve all the remains of the Hieroglyphical Inscriptions and Manuscripts which have hitherto escaped the injuries of time. For this purpose, the efforts of a single individual would probably be too feeble, and the duration of a single life might possibly be too short: but it may be effected with much more ease, and with far greater certainty, by the continued co-operation of a select Society determined to keep it constantly in view.

“The process of lithography affords a ready mode of obtaining a moderate number of copies of a drawing at a cheap rate. The object of this collection being to exhibit perfectly correct representations of the greatest possible extent of materials for a limited sum, the introduction of any unnecessary ornament would obviously be inconsistent with its complete attainment; and the delineation of all works of art, as such, must, for the same reason, be excluded:

“It will naturally be desirable to select, in the first instance, in order for their permanent preservation, such inscriptions and manuscripts as have not yet been published; but it is intended that the work should ultimately comprehend every thing of the kind that can be obtained; not only because some of the most important materials are thinly scattered through a variety of magnificent and expensive works, but also because such a collection would afford a very great convenience, both for study and for reference, even to those who are already possessed of the original works which contain them.

“In order to avoid the introduction of arbitrary hypotheses and erroneous conclusions, no commentaries, nor even any particular nomenclature, will be admitted into this series of hieroglyphics. It was indeed in contemplation to have begun the work with a copy of the Inscription of Rosetta, subdivided, and having the parallel passages of the three texts printed together, according to the arrangement of the anonymous translation published in the *Archæologia*; but it has been thought more advisable to defer this

comparison, in the hope that some of the duplicates of the stone, which have remained more entire, may speedily be obtained from Egypt.

“The general subjects of the hieroglyphical inscriptions which they contain, may be collected from an article on Egypt, which is about to appear in the Supplement to the *Encyclopædia Britannica*. The first six exhibit a tolerably perfect specimen of the manuscripts frequently found with mummies, and which always contain a series of homages addressed to the different deities in the name of the deceased. The next subject consists of friezes brought from Egypt, and now in the British Museum, compared with another fragment of the same series found in the ruins of Rome. The colossal head, which has lately been presented to the British Museum in the names of Mr. Salt and Mr. Burckhardt, occupies the greater part of the 10th plate; and the subjects delineated in the five following plates are more or less immediately connected with this figure, exhibiting either the name, which is still distinguishable in the inscription on the back, or that of Memnon, whom the head has sometimes been supposed to represent, or some other name approaching very near in its form to one or the other of these two.

“The execution of the work is so arranged as to afford the subscribers the greatest possible benefit for their contributions; and *not only the whole of the money collected will be employed for defraying the expenses, but some further voluntary assistance may be expected* from individuals; a nobleman who has travelled in Egypt having already set the example by taking upon himself the expense of the drawings of a valuable hieroglyphical MS. which he has lately received from the British Consul at Cairo.

“Each subscriber will be required to pay one guinea in advance at the time of subscribing, and two guineas annually upon the receipt of each volume, which will probably contain from 20 to 50 folio plates.

“No copies will be sold, except to those who may become subscribers at a future time; and in such cases the amount of the sale will be carried to the account of the Society, of which an annual statement will be laid before the subscribers. A copy will be deposited in the British Museum, another in the King's Library at Paris, a third in the Vatican, and a fourth in the Academical Library of Göttingen. Other public libraries will be admissible as subscribers, it not being intended to limit in any manner the description of persons subscribing, nor the number of copies which they may wish to take.

“The management of the work, and any further proceedings of the Society, which may be thought advisable, will rest entirely with the Directors, who will also have the power of making, from
time

time to time, such additions to their own number as they may think proper. For the present, Taylor Combe, Esq., William Hamilton, Esq., Lieut.-Col. Leake, the Earl of Mountnorris, and Matthew Raper, Esq., have undertaken the responsibility of this office.

“Subscriptions will be received by Mr. Yeoman, Collector to the Society, No. 3, Lincoln’s Inn Fields.”

THE ROYAL ACADEMY OF INSCRIPTIONS AND BELLES LETTRES,
PARIS,

Has proposed the following prize subject for the year 1821:

“To compare the monuments which remain of the ancient empire of Persia and Chaldea, either edifices, basso-relievos, statues, or inscriptions, amulets, coins, engraved stones, cylinders, &c., with the religious doctrines and allegories contained in the *Zend Avesta*, and with the indications and data which have been preserved to us by Hebrew, Greek, Latin, and Oriental writers, on the opinions and customs of the Persians and Chaldeans, and to illustrate and explain them as much as possible by each other.”

The prize is a gold medal of 1,500 francs value. The essays are to be written in Latin or French, and sent in before the 1st of April 1821. The prize will be adjudged in July following.

THE ROYAL ACADEMY OF COPENHAGEN

Proposes the following prize questions:

Mathematics.—“Nùm inclinatio et vis acus magneticæ iisdem, quibus declinatio diurnis variationibus sunt subjectæ? Nùm etiam longiores, ut declinatio, habent circuitus? Nùm denique has variationes certis finibus circumscribere possumus?”

“Quibus naturæ legibus regatur primaria evolutio corporum animalium, ut formam sive regularem sive abnormem assumant.”

The prizes attached to these subjects are 50 Danish ducats.

Geology.—“Quæ saxa ad montes ordinis secundi, seu transitorios, pertinentia in Norwegia reperiuntur?”

This prize, proposed by His Excellency S. G. Moltke, is of the value of 550 rubles. The memoirs are to be written in Latin, French, English, German, Swedish, or Danish, and should be directed to M. H. C. Orsted, Secretary to the Academy, by December 1819.

DIJON ACADEMY OF SCIENCES, ARTS, AND BELLES LETTRES.

This Academy has proposed the following question as the subject for the prize to be awarded in 1820.

“What may be the most effectual means of extirpating from the hearts of Frenchmen that moral disease, a remnant of the barbarism of the middle ages, that false point of honour which

leads them to shed blood in duels, in defiance of the precepts of religion and the laws of the state?"

The same Academy has proposed the following question for 1821.

"How far is it possible, in the present state of philosophy, to explain aqueous meteorological phenomena?"

LXVIII. *Intelligence and Miscellaneous Articles.*

AURORA BOREALIS, OR NORTHERN LIGHTS.

A SINGULAR and striking modification of that very mysterious phenomenon, the *aurora borealis*, presented itself to the inhabitants of all the northern parts of England and the south of Scotland, on the evening of Sunday the 17th October, of which we have received some particulars from various places. The following is the account that has been transmitted to us from an ingenious Mine-engineer, who happened at the time of its appearance to be residing at *Seathwaite* in Borrowdale, in Cumberland, at the house of Mr. Dixon, the resident Agent of the black-lead mine; viz.

Monday 18th Oct. The wind here yesterday morning blew rather strong and cold from the north, but the same died away as the sun advanced in its course, through a fair and nearly clear day; so that the evening after sun-set proved exceedingly still, clear and starlight, with but a moderate degree of cold, compared with that of the morning. In the evening, at about ten minutes before nine by the clocks here, (which perhaps may differ considerably from true time,) I was called out of the house by Mr. Dixon, who a minute before had walked out, to witness a singular appearance in the sky, which seemed just then beginning to show itself. On going out, I observed an unusual degree of light shining over the tops of all the crags which surround this very deep and sudden valley; particularly over the high hills almost due north from us, called Brunslow-How; over which, full as vivid a light shone as from the twilight of a midsummer's night. This general glimmer of light in the horizon (which we referred without hesitation to the aurora) occasioned the outlines of all the crags and hills forming our visible horizon, to be almost as well defined as by day-light some time before or after sun-rising or setting, and enabled Mr. Dixon to observe and to name the very particular parts of the eastern end of Bas-Brown hill, to the westward, and those of Jenny-bank Crag to the eastward, between which points of our horizon a narrow, and nearly equally wide stripe of thin white and slightly luminous cloud was stretched, not in a straight vertical plane, but apparently in a
plane

plane which inclined over to the southward at top several degrees, perhaps 10 or 20 degrees; through which luminous arch, which had about the apparent breadth of the ordinary rainbow, all the larger stars in its course were plainly visible, although the Pleiades or seven stars but faintly appeared through it, at a small elevation above the crag on the east: all the other parts of the sky appeared perfectly free from clouds or haziness, and there was no appearance of the streaks or flashes of reddish coloured light, which usually distinguish remarkable displays of the aurora borealis; which to us showed itself only by this light in the visible horizon, but which no where extended far above the same. On watching attentively this curious and singular arch, it visibly declined in intensity towards the eastern end, and this diminishing of its appearance extended gradually towards the western end, until, only about one quarter of its course remaining distinctly visible, at nine o'clock we went into the house, considering the whole appearance to be nearly ended.

At 9^h 15^m we however went out again, and, to our great surprise, observed the luminous arch to be renewed, broader than before, but scarcely so well defined at the edges, or so dense; so that smaller stars were visible through it now, than before; and instead of its former inclining position, its two ends had now moved 12 or 13 degrees more northerly upon the hills bounding our sight; and on holding up a plumb line, the arch appeared now to pass through our zenith, or to lie in a vertical plane, exactly in the magnetic east and west, as has appeared this morning, by taking the bearing, with a good compass, of the particular parts of the crags which Mr. Dixon was able to recognise. There was, as before, no other appearance of clouds in the sky; but on watching attentively the northern edge of the arch, near to the zenith, it appeared to send off five or six short branches of the luminous matter, not at right angles to its plane, but directed towards the NW. We again went into the house, until 9^h 30^m, when, again observing, the two ends of the luminous arch were now moved back, very nearly, to their former places on the crags, and the upper part of the arch again inclined, (or *haded*, to use a miner's term,) towards the S, apparently as much as before; the breadth was now not greater than at first, but it was on the whole fainter, and smaller stars were visible through it: by degrees it now began to disappear, first at the western end; and at 9^h 35^m, when our observations ended, only a small portion thereof was visible at the eastern end.

Mr. Dixon's son (John) being at Rosthwait, about two miles lower down the valley to the NE, where the same meets with the larger valley of Stonethwaite running near S and N, he observed in the more open and distant visible northern horizon of

that place, strong marks of the aurora, moving and flashing about. He did not begin his observations until the luminous arch was in its second or vertically E and W position, by the needle; but after that, his observations exactly corresponded with ours, in tracing the arch from its vertical to its southwardly inclined and more southern position, and until it finally vanished in the east.

The most curious and important part of the above observations, are perhaps those which relate to the visible approach of the arch towards the north, and then returning back to its former position.

[From another Correspondent.]

To Mr. Tilloch.

SIR,—The reappearance of the aurora borealis in Britain after so long an interval, being an object of curiosity to many of your meteorological readers, I submit the following short notice thereof, for insertion at your pleasure.

On Friday the 15th of October the northern lights were distinctly seen from Barton mills, Suffolk, by several persons of my acquaintance; among others by Mr. B. M. Forster, of Walthamstow, while travelling into Norfolk by night.

On Sunday the 17th of the same month he observed this phenomenon again, from Schole inn near Diss, about 8 P. M., and describes it as exhibiting a very brilliant light of a pyramidal figure.

It appears that on the same evening the aurora borealis was seen from Shoreditch and from Camberwell, and in other parts of the neighbourhood of London. Another correspondent has noticed the phenomenon on the same evening at Carlisle.

If any correct observations in different parts of the country should have been made on the aurora, the communication of them in the Philosophical Magazine would probably amuse many of your readers. As yet there have been but few observations on this phenomenon accurately recorded. The reader may refer to Dalton's Meteorological Essays, to Davy's Elements of Chemistry, and particularly to a curious work by Mairan, sur l'Aurore Boreale, 4to. Paris 1754, in which several new views of the possible cause of this curious appearance are to be found, together with an account of the observations made in France on the zodiacal light. I am Sir, yours,

Nov. 10. 1819.

T. FORSTER.

DREADFUL EARTHQUAKE IN THE EAST INDIES.

[From the Bombay Gazette of July 7.]

“Camp Bhooj, June 19.

“At seven o'clock on the evening of the 16th of June, an earthquake destroyed the whole district and country of Kutch. Accounts

counts that have been received mention, that from Luckput Bunder to Butchao, the whole of the towns and villages are more or less in ruins. The towns of Mandavie, Moondria, and Anjar have suffered extensively and severely; but the city of Bhooj and the fort of Bhoojia, between which our force is encamped, are reduced the former to ruins, and the latter so breached as to be useless as a fortification. This, however, is the least part of the evil: at the moment of the crash, it is apprehended, and I fear not any way exaggerated, that 2000 of the inhabitants were buried in the mass.

“Even now the effects of this horrible visitation are felt, though three days since the first shock, in constant and hourly vibrations of the earth. The inhabitants have been obliged to forsake what were once their halls, and encamp outside upon some small hills. Their distress cannot be well described—bruised, maimed, and agitated with fear, they go daily into the city to work upon their several houses, and try to extricate the mangled remains of wives, children, and relations, whilst in their pious labour the putrid stench nearly exhausts them; and cattle, which have fallen in numbers, add greatly to the noisome evil. The walls, from the sandy nature of the stone, are crumbled in a mass, and the narrow streets of Bhooj entirely lost, thus adding to the difficulties of the sufferers.

“The upper stones of the palace fell, and buried, amongst others, the mother of the deposed Rao; what houses stand are so shattered as to be liable to fall in the ruins; and the very complete wreck of the wall on the southern side, as well as the demolition of nearly all the towers and gateways, render it impossible for Bhooj to be a city again.

“The loss of lives cannot be confined to the city. I fear, in all the towns and villages mortality has been great. I am inclined to think, from the circumstance of a volcano having opened on a hill thirty miles from Bhooj, that the country will experience a repetition of the evil.

“From our camp being in a plain, no very material damage has been sustained: the tiles of a few temporary erected houses were knocked off, and the walls shattered.

“I shall attempt to give you the sensation felt by those both in camp and city. In the latter, I was informed by a gentleman, who nearly suffered by a house falling over him, that riding on, without an idea of what was to happen, upon the first notice, a heavy appalling deadened noise, the motion of the earth, walls of the houses on each side of the street tottering and falling outwards, impressed upon him an idea, and he called out, that a mine was sprung; whereas, another gentleman imagined the bank of the tank was forced by the water: these ideas were accompanied with

an unpleasant giddiness of the head, and sickness of the stomach, from the heaving of the ground :

——‘ In one wild roar expired!
The shatter’d town, the wall thrown down,
The waves a moment backward bent ;
The hills that shake, although unrent,
As if an earthquake pass’d !’

Byron’s *Siege of Corinth.*

“In camp a similar sickness and giddiness was experienced; and in ignorance until the shock was over, which lasted a minute, of the nature of the noise below the earth, some sat down instinctively, others threw themselves down. One was paying work-people in a circle, and upon seeing him squat, the whole followed the example, and sat round him—the very picture of despair. The sensation I felt was a giddiness and horror at perceiving a small hillock, close to which I was riding, a short distance from the camp, completely agitated, and at the same time my horse plunged, from the ground moving. This was the case also with an officer I was riding with. I have, on inquiry, ascertained that many years ago, and in the remembrance of the oldest inhabitants, an undulating motion has been felt before in Kutch, but never, I hope, will again be attended with such a horrible catastrophe; the distress of which has been so great upon the inhabitants, that I confess I fall short of ability to describe it.”

EARTHQUAKE.

On the 26th of May a violent shock of an earthquake was felt at Corneto in Italy, which damaged several houses, but happily without any loss of lives. The celebrated ancient Gothic cupola of Castello was thrown down, and the church of the Minor Friars, of which it formed a part, was greatly damaged. This shock was felt along a great part of the coast of the Mediterranean.

COMETS.

It is now ascertained that one and the same comet returned to our system in 1786, 1795, 1801, 1805, and $18\frac{1}{9}$. It appears that it never ranges beyond the orbit of Jupiter. Its short period, of little more than $3\frac{1}{4}$ years, and its mean distance from the sun, which is not much greater than twice that of the earth, connect it in a particular manner with the part of the system in which we are placed: of course, it crosses the orbit of the earth more than sixty times in the course of a century. Its elements as seen in $18\frac{1}{9}$ are as under.

Passage of perihelion, mean time at Gotha, Jan. 27, 28977
Longitude of perihelion $150^{\circ} 59' 15''$
Longitude of node $334 \quad 35 \quad 0$
Inclination

Inclination of orbit	13° 37' 0"
Angle of eccentricity	58 2 58
Logarithm of half the greater axis	0.34500
Half the greater axis	2.2131
Period	1202.54 days.

From these elements it appears that this comet is at present in opposition to the sun, and may perhaps be seen by very powerful telescopes.

According to the calculation of M. Olbers of Bremen, after a lapse of 83,000 years, a comet will approach to the earth in the same proximity as the moon; after 4,000,000 years it will approach to the distance of 7,700 geographical miles; and then, if its attraction equals that of the earth, the waters of the ocean will be elevated 13,000 feet, and cause a *second deluge*. After 220,000,000 years, it will clash with the earth.

THE PLANET PALLAS.

According to Derksen, the next opposition of this planet will take place in 1820, January 6, at 20^h 16' 41" mean time at Gottingen: Longitude 106° 0' 16".2, Geocentric latitude 54°. 28' 33".2 S.

ANTIQUITIES.

Some time ago, in digging to make gas tanks at the Low Lights, near North Shields, in a place called Salt Marsh, in Pow Dean, at the distance of 12 feet 6 inches from the surface, the workmen came to a framing of large oak beams, black as ebony, pinned together with wooden pins or tree-nails: the whole resembling a wharf or pier, whither ships drawing 9 or 10 feet water had come. Muscle shells lay under an artificial spread or coating of fine clay, as in the bed of a river. Julius Agricola, about the 83d year of the Christian æra, had his fleet in the Tyne; but tradition says, he moored them in the brook Don, near where Jarrow Church now stands; he may have also moored some of them in this place (opposite to the Roman station, near South Shields), as it has been a secure estuary at the mouth of the Pow Bourne, guarded from the sea by a peninsula of clay and sandy land, now called the Prior's Point, whereon Clifford's Fort was built in 1672. Large oak trees were also found, hollowed out as if to convey water. Had there been found any scorïæ, or calcined stones, conjecture might have pointed to salt-works having been here; but, on the contrary, few stones were found, only sandy black mud 12 or 13 feet deep, and one freestone, squared out in the middle to hold the foot of a wooden pillar: hammer marks were visible in the sides of the square hole. On the side of the peninsula above referred to, next to the estuary, salt-pans were working

ing in the time of the Priory at Tynemouth; probably as early as the year 800, and so to the dissolution in 1539; and according to Brand, and other records belonging to the Duke of Northumberland, the Pow Pans were making salt in the reign of Elizabeth; and in 1634, the Corporation of the Trinity-House, Newcastle, bought land near Tolland's, Delaval's and Selby's Pans, to erect their Low Lights upon. Much of the oak moulders away on being exposed to the open air: but some beams and planks are preserved, out of which it is intended to make chairs, &c. The Danes often moored fleets in the Tyne, during their excursions, in the ninth, tenth, and eleventh centuries.—(*Durham Advertiser.*)

EGYPTIAN ANTIQUITIES.

The following is an extract from a private letter from a gentleman of talent and acquirement, who is at present engaged in visiting the monuments and curiosities of Egypt.

“Cairo, March 4, 1819.

“Our Italian expedition has terminated in the most favourable manner. We arrived here yesterday from the Upper Cataracts, after an absence of four months, without having experienced on our way any kind of difficulty whatever. I found Egypt equal to its fame, and far surpassing in the importance of its architectural and sculptural remains, in connection with the history of the two Arts, any opinion I had collected from previous travellers. Volney says judiciously—‘*Nos jugemens sont bien moins fondés sur les qualités réelles des objets, que sur les affections que nous recevons, ou que nous portons déjà en les voyant;*’ but this tacit censure of all descriptions can scarcely apply to one of this country, where the strongest tests of its greatness are the strong emotions produced by its ruins. Our whole journey, to me at least, was a series of successive pleasure; and I am at a loss to say whether I was the more astonished by the grandeur or numbers of its monuments.

“We left Cairo in November, and proceeded very rapidly up the river to Dendera. The Temple is one of great magnitude, and is, perhaps, in a more perfect state than any other monument in Egypt. We remained here four entire days, occupied from morning till evening with the measurements and other details of the architecture and sculpture. The northerly winds prevailing at this time of the year, and not being willing to lose any opportunity which they offered us, we did not delay at Thebes, but passed it rapidly a few days after our departure from Kerouch, almost immediately opposite Dendera. The first view of this extraordinary city, now split into five distinct villages, is equal to the warmest panegyrics of Denon, and no praise too large can be given to the greatness and sublimity of the combinations, architectural and natural,

natural, which it presents. A few calm days, with oppressive south winds, detained us some time below Errouan, on the First Cataracts: we reached them in December. The necessity of changing our boat, the large one in which we came up the Nile to Errouan being too heavy for the shallows above the First Cataracts, at this time of the year particularly, we were obliged to remain at the small island of Philæ, a few miles from Errouan, three or four days in succession. This time was well occupied in making sketches, &c. of the various buildings of the island, arranging notes, &c. Late in December we recommenced boating, and proceeded on our way through the ancient Ethiopia. The remains here are still more perfect, perhaps, than those of Egypt, being, with the exception of the excavated temples, referable to a late period (the Ptolemaic dynasty), and not subject to the frequent injuries of successive occupiers. Comparing the physiognomy of the present race with that usually adopted in all their paintings, of which great and well preserved specimens are to be found in almost every temple, it is almost apparent that very few changes, if indeed any, have occurred, and that the Nubians of the day are the descendants of the ancient Ethiopians. We made our Christmas dinner in the capital of the country, Deim; but you are not to understand by these high-sounding appellations any thing more than a third or fourth rate kind of Irish village. The inhabitants are worthy of their works—wretchedly dwarfed in all the fair proportions of mental and bodily strength, and as contemptible in character as in appearance. I have generally found mind gradually decreasing as I proceeded south, with all other high qualities, beginning with England as the maximum; but I am not altogether inclined to propose the assertion without some qualifications. We met in our return some Seneer men, very far superior in all particulars to the miserable population of this country. On the 2d of January we attained the limits of our journey, and remained a few hours at the Upper Cataracts, beyond which all navigation ceases: we read the names of hamlets, looked once more south towards the blue mountains of Dongola, and returned across the Desert to our boat.

“We had for a short time serious intentions of penetrating still further towards the equator; but the unimportance of the very few ruins which remain, not more than three temples, and the difficulty of procuring camels for so large a party, deterred us, on more mature consideration. We returned a day or two after, to Abouranbol, the principal temple in Ethiopia: it is excavated in the solid rock, and of a simplicity, magnitude of dimension, and solemnity, even eyes familiar with ordinary Egyptian works have not been accustomed to. We found that the excavation made at the head of the door a year and a half ago, by Captains Man-
gles

gles and Irby, Signor Belzoni, &c. who were the first who entered it, had been already closed by the accumulation of the sand, which pours down like a torrent from the Desert; and we had forty or fifty men, besides ourselves and servants, occupied for two or three days in re-opening it. The entrance well repaid all or any labours which could be undertaken for the purpose. Imagine the effect of six colossal figures, of a size beyond any thing to be seen in Europe, attached to six huge pilasters on each side of the first great apartment or portico of the temple. This chamber is succeeded by a variety of other smaller ones, connected with or preceding the sanctuary, some supported with pilasters, others without, but richly decorated with mysterious and original sculpture and painting, illustrative of the religion or history of the achiever. The front has no pillars, and hardly any other embellishment than four sitting statues reposing against its face, the proportions of which may be loosely determined from the measurement across the heart, 28 by 8. These figures are perfectly well executed; and though the model chosen is certainly not very consistent with our standard of real or ideal beauty, it is very consistent with itself, and the general result productive of a very noble impression. It stands immediately on the Nile, and is to be seen at a great distance. In addition to this, as its final praise, I may say that these are the only colossal statues that do not lose on approach: those of the *Memnonium* at Thebes, and particularly the great sitting statues, disappointing both the eye and imagination as you advance. We returned to Errouan towards the end of January, and resumed our labour at Philæ. Denon places it so incorrectly, that you would hardly recognise in the outlines or proportions the position or character of these ruins. We spent more than two days in planning the whole island anew; on the accuracy of which you may safely rely, as I imagine the artist who accompanies us, and is very intelligent, has not omitted the measurement of a single angle or distance in the whole circuit of the place."

TO PREVENT SMUT IN WHEAT.

Liming the seed, by immersion, is recommended (in the *Bibliothèque Physico-économique*) as the only preventive warranted by science and sanctioned by experience; and the following is given as the method in which the process is best performed:—we employ the English denominations.—To destroy the germ of the blight in $4\frac{1}{2}$ bushels, or 256 pounds of corn, about six or seven gallons of water must be used, as the grain may be more or less dry, and from 35 to 42 ounces avoirdupoise of quick-lime, according as it may be more or less caustic, and according as the seed may have more or less of the blight. Make part of the
water

water to boil, slack the lime with it, and then add the rest of the water. When joined, the heat of the water should be such that the hand could with difficulty bear it. Pour the lime-water upon the corn placed in a tub, stirring it incessantly, at first with a stick and afterwards with a shovel. The liquid should at first cover the wheat three or four fingers breadth: it will soon be absorbed by the grain. In this state let it remain, covered over, for 24 hours; but turning it over five or six times during the day. Such of the liquor as will drain off is then to be separated; when the corn, after standing a few hours, in order that it may run freely out of the hand, may be sown.—If not intended to be used immediately, the limed wheat should be put in a heap, and moved once or twice a day till dry.

Experience has proved that limed grain germinates sooner than unlimed; and as it carries with it moisture sufficient to develop the embryo, the seed will not suffer for want of rain; insects will not attack it, the acrid taste of the lime being offensive to them: and as every grain germinates, a less quantity is requisite. In fact, the grain being swelled, the sower, filling his hand as usual, will, when he has sown 65 of limed corn, have in reality only used 52.

As blighted grains preserve, for a long time, the power of germinating; the careful farmer whose corn has been touched, should carefully sweep out the crevices in the walls and cracks in the floors of his barn, and take great pains to clean them thoroughly: for the blight is so highly contagious, that from a single grain will grow enough to blight an ear, which in its turn may infect a field, and at length cause the loss of the crop of a whole country, as was the case [in France] in 1784 and 1788.

TO PREVENT MILDEW IN WHEAT.

Salt one part, water eight parts. With this mixture sprinkle the diseased corn. Where the corn is sown in drills, this may be done with a watering-pot; but the best and most expeditious mode is with a flat brush, such as white-washers use, having a tin collar made water-tight round the bottom, to prevent the mixture dropping down the operator's arm, and running to waste. The operator having a pail of salt and water in one hand, and dipping the brush into the mixture with the other, makes his regular casts, as when sowing corn broadcast; in this way he will readily get over ten acres in the day. About two hogsheads will do one acre: wherever the mixture touches, in three or four days the mildew will disappear; upon those parts that escape, the sprinkling must be repeated. If judiciously cast, the mixture falls in drops as uniformly as rain.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Archer Deacon, of Pilgrim's Hatch in the parish of South Weald, Essex, gentleman, for certain improvements in the manufacture of boots and shoes and clogs, by the application of certain materials hitherto unused for that purpose.—1st November 1819.

To Sir William Congreve, of Cecil-street, Strand, in the county of Middlesex, bart., for his improved mode of enlarging or combining different metals or other hard substances applicable to various useful purposes.—1st November.

To Israel Gundy, gent., Edward Neave and Josiah Neave, shopkeepers, all of Gillingham, Dorsetshire, for their application of various gases or vapours to certain useful purposes.—1st November.

To William Hudson, of Cranbroke, Kent, for improvements in the manufacture of boots and shoes.—1st November.

To Samuel Shorthouse, of Dudley, Gloucestershire, for a machine to cut straw of any length required, thereby rendering straw a better and more convenient winter food for cattle, and the manure produced in farm-yards, &c. fit for immediate use; also for rendering dry straw a manageable manure; also for cutting straw to mix with horses' corn; also for cutting straw to any given length for any other purpose.—1st November.

To John Heard, of Birmingham, for his improvements on cooking apparatus.—4th November.

To John Grafton, engineer to the Edinburgh Gas Light Company, for his new and improved apparatus for purifying gas used for illumination.—18th November.

To Louis Fauche Borel, of the Haymarket, in the parish of Saint Martin in the Fields, gent. for an invention called The moveable and inodorous conveniencer.—16th May.

To John Sinclair, for a new method of introducing coloured threads into flowers and other fancy figures in the process of weaving, whether the said articles are made of silk, cotton, worsted and hemp, or mixtures thereof.—18th November.

To Joseph Glenmy, of Saint John's square Clerkenwell, and John Darby of Gee-street, Middlesex, for a machine and apparatus calculated to answer the purposes of a fire and burglary alarm.—23d November.

To George Lilley, of Briggin, Lincolnshire, for certain improvements in the construction of engines or machinery (to be wrought by steam or other elastic fluids) applicable to the driving of mills, and other useful purposes.—23d November.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1819.	Age of the Moon.	Thermo- meter.	Baro- meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
Oct. 15	26	58°	30·10	Fine
16	27	52°	30°	Ditto
17	28	45°	30·10	Frequent squalls with rain
18	29	51°	30·08	Cloudy
19	new	55°	29·80	Ditto
20	1	54°	29·30	Rain
21	2	40°	29·48	Cloudy
22	3	43°	29·20	Ditto—brisk wind—a considerable fall of snow early in the morning, which remained on the ground till noon.
23	4	50°	29·10	Ditto
24	5	47°	29·25	Stormy
25	6	46°	29·30	Rain
26	7	47°	29·52	Cloudy
27	8	46°	29·74	Fine—rain in the morning
28	9	44°	29·70	Cloudy
29	10	44°	29·52	Rain
30	11	47°	29·60	Cloudy
31	12	50·5	29·80	Ditto
Nov. 1	13	49°	29·64	Rain
2	full	45°	29·50	Fine
3	15	44°	29·77	Ditto
4	16	54°	29·66	Cloudy
5	17	56°	29·37	Ditto
6	18	49°	29·14	Fine
7	19	46°	29·30	Ditto
8	20	43·5	29·46	Cloudy
9	21	43·5	29·66	Ditto
10	22	46°	29·16	Rain
11	23	47·5	29·70	Fine—heavy rain P.M.
12	24	47°	29·80	Ditto—rain in the morning
13	25	47°	29·70	Ditto
14	26	43°	29·69	Cloudy

METEOROLOGICAL TABLE,
 BY MR. CARY, OF THE STRAND,
 For November 1819.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock Morning.	Noon.	11 o'Clock Night.			
Oct. 27	34	43	40	29.84	0	Rain
28	36	46	35	.84	0	Cloudy
29	36	44	40	.52	0	Rain
30	44	45	40	.62	0	Rain
31	48	48	46	.90	15	Cloudy
Nov. 1	46	48	40	.85	22	Cloudy
2	40	47	40	.77	26	Fair
3	38	48	41	30.01	32	Fair
4	42	54	46	29.99	21	Cloudy
5	47	54	46	.72	20	Cloudy
6	47	53	44	.52	26	Fair
7	42	52	40	.52	28	Fair
8	37	45	44	.66	24	Cloudy
9	32	45	44	.87	26	Fair
10	46	50	40	.37	29	Fair
11	45	44	42	.79	0	Rain
12	40	47	44	.99	24	Fair
13	44	46	45	.82	16	Cloudy
14	43	46	45	.85	12	Cloudy
15	45	47	44	.77	10	Cloudy
16	46	42	37	.51	0	Rain
17	39	44	42	.85	0	Rain
18	44	44	35	30.10	22	Fair
19	33	39	37	29.95	15	Cloudy
20	35	39	43	.57	23	Fair
21	39	44	35	.20	16	Cloudy
22	33	40	32	.58	21	Fair
23	31	39	31	.84	20	Fair
24	27	39	35	.99	19	Fair

N.B. The Barometer's height is taken at one o'clock.

LXIX. *Continuation of the Reply to Mr. RIDDLE's Remarks on Mr. MEIKLE's Paper "On the Lunar Observations."* By Mr. MEIKLE.

To Mr. Tilloch.

SIR, — **I**N your last Number I hope I have cleared my paper on the lunar observations from some of the groundless charges of Mr. Riddle. There still, however, remain several other things to be noticed. For the most part Mr. R. has been at great pains to prove trifles which every body knows, and which I never denied. But passing over this at present, I come to consider the learned demonstration which he employs to prove that my method of finding the true altitude of the centre is not more accurate than that method in which the *reduced* semidiameter is used*. With regard to this, I need only observe, that if he had given my paper a fair reading, he might have seen plainly that it is not the method with the *reduced* semidiameter "on which I animadvert with so much severity," (page 35, line 28,) but the common slovenly method almost in universal use, in which the *reduction* of the semidiameter is not used at all; and at the altitude of 7° this reduction on a mean amounts to $18''$, which is just the error complained of. I there called it a *needless* error, and so I do still; for the very accurate method which I recommended, is attended with as little labour as the most slovenly method can be.

Again: In giving his "vulgar test of arithmetical computation," he does very unfair to compare my method with what he calls "the common method." Now, in the common method, as every body knows, the reduction of the semidiameter by refraction is neglected altogether; and it was for this very neglect that I "animadverted on it with so much severity." I must however remark, that I have never, throughout my whole paper, although Mr. Riddle would fain insinuate it, inculcated the observance of such nicety at sea; where, as is too well known, some seamen are tolerably content if they get the altitude within half a degree of the truth; and for such, the common method is certainly more than sufficiently correct. But still, when at any time persons, who are not aware of its incorrectness, make a landing, and attempt to determine their geographical situation, it must of course be erroneous. It was for this very reason that I so deservedly censured the method of finding the latitude by the pole star. In giving an approximation, it is surely the least thing an author can do, to mention that it is not theoretically correct,

* That the apparent altitude of the *centre* may also be got correctly by this method, is what I never once called in question.

and within what limits it may be used with safety. It is undoubtedly owing to some such causes as the above, that one navigator finds water where another placed the land; or that islands have been known to dive and swim alternately at pleasure.

It must, no doubt, be acknowledged that another person will often discover what the author of a paper did not himself observe; and in this respect Mr. R. has shown much acuteness, since most of the things he has discovered do not exist in my paper at all. In order to set me in as odious a light as possible, he says, "It would indeed appear that I am not aware that the *central distance* of the objects is what is required." But it is past his power to produce the "appearance" of any such thing; and after all the contempt he affects to throw on the "eccentric point," he is not able to show that it will ever produce any "error" whatever. The rule however which I gave on page 37, is plainly meant as approximate, and not as strictly correct in low altitudes.

In perusing my paper in your last Number, which was written in too great haste, I am really sorry to find that I have expressed myself very incorrectly about the augmentation of the moon's diameter. I was led into that mistake by adhering too closely to Mackay's remark, which Mr. Riddle says is "theoretically true;" whereas it is no such thing, as he may soon find at his leisure. What I have said about the augmentation being greatest in a vertical direction is entirely a mistake; but as the error, and indeed the whole dispute, is merely about a small fraction of a second, it is useless to say any thing further about a quantity that can do neither good nor ill.

With regard to what I formerly advanced respecting the principles of the quadrant, and on which Mr. R. animadverted with so much groundless severity, I would beg to observe, that in explaining the principles of any instrument, it is certainly of the last importance to assign to each particular part its proper use or effect; otherwise, it does not deserve the name of an explanation at all; and can at best convey vague or erroneous ideas of the nature of the instrument. Had Mr. Riddle himself been the first to discover the popular mistake in question, that would have *altered the case*. But it would indeed have been too much for any other person but himself to have had the merit of discovering "that a mistaken idea was generally entertained respecting the theory of an instrument of such importance."

I shall now proceed to demonstrate that the halving of the observed angle is completely effected by the first, the *single* reflection; and in so doing, I hope I shall not be taxed with producing a demonstration of the principles of the instrument which is
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to be had in every corner, as Mr. R. has done with great ability in your Number for October, although it did not in the least suit his purpose. For the original intention of that demonstration was merely to show that the instrument only gives half the observed angle; and on no account to prove, what is not true,—that the halving is owing to the *double* reflexion. To talk of the "actual construction of the instrument" is nothing to the purpose; for that construction includes several other glasses which are just as much concerned in halving the angle as the horizon glass is. Indeed we might on the same grounds ascribe that effect to the frame's being constructed of wood or brass. But the grand thing to be kept in mind is, that the "instrument as actually constructed and used," involves nothing on which the halving of the angle depends but the *single* reflexion of the index-mirror.

Let MI be the position of the index at zero, or when the altitude of an object $\odot$ is $= 0$; and let $\odot M$ a ray from the object impinge against the mirror M , and be reflected to R , making, by the principles of optics, the angle of incidence $\odot MP = PMR$ the angle of reflexion.

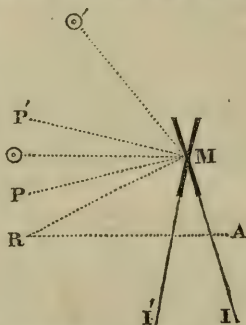
Suppose now that the object has attained a certain altitude $\odot M \odot'$, and that we turn the index I till the ray $\odot' M$ is reflected to R , making as before $\odot' MP' = P' MR$.

It is then evident that the angle $PMP' = P' MR - PMR = \frac{1}{2} \odot' MR - \frac{1}{2} \odot MR = \frac{1}{2} \odot' M \odot$. But $PMI = 90^\circ = P' M I'$; from each take PMI' , and $I' M I = P' M P = \frac{1}{2} \odot' M \odot = \frac{1}{2}$ observed angle.

The same thing may be shown in several ways; but since the halving of the observed angle is thus completely effected by the first, the *single* reflexion, it is clear that the only use of the second mirror fixed at R , is to reflect the ray MR in a direction parallel to $\odot M$; so that an observer at A might see the object in contact with the horizon.

Each mirror, therefore, serves as distinct a purpose from that of the other, as it is possible for any two mirrors to do in one instrument; and to confound the use of the one with that of the other, is to explain a simple principle into a mystery, where no real mystery exists.

Upon the whole, Mr. Riddle is certainly very bright on the quadrant. "It is true," says he, "that if a ray of light be reflected



flected from a revolving mirror, the angle described by the reflected ray will be double of that described by the mirror,"—a statement meaning neither more nor less than that the halving of the observed angle is completed by the *single* reflexion of that *one* revolving mirror. And yet in a little after he would like to prove the contrary; but on account of the impossibility of the task, he contents himself with trying to palm the popular error in question, on the demonstration he had copied from some elementary work; and, strange to relate, he at length brings out the extraordinary conclusion, that there is nothing "vague or insignificant" in ascribing the above effect to the "*double* reflexion!"

Mr. R. lastly admits, though reluctantly, that I had correctly stated the error of certain tables*, but that I had "disingenuously converted that into an argument against putting confidence in any tables of the kind." Now this accusation is totally unfounded; since it is to his own candour and moderation that I owe its extension to "any tables of the kind." My own words are:—"It is not difficult to perceive what confidence ought to be put in such tables;"—obviously meaning the tables only which contained the gross error, and by no means any tables of the kind. It is then Mr. R. himself who "has betrayed the aberration from rectitude of intention," by disingenuously laying so many erroneous things to my charge, which he might have been content to have attributed to his own misconstruction.

I am, sir,

Your most obedient servant,

Berners street, Dec. 3, 1819.

HENRY MEIKLE.

P. S.—In your last Number, *Mathematicus*, among other things, accuses me of advancing three distinct properties of the ellipsis "with an air of novelty." Now that this accusation, so far as "novelty" is concerned, must be very incorrect, is obvious to any one who merely looks at the uncertain mode in which I have announced that proposition.

With regard to my other crime of not reading, I would beg to inform him, that I had turned over several very complete treatises on Conic Sections, some of those he mentions not excepted; but then I was only in quest of the other two properties, without concerning myself at all about the third "long known" one, until writing out the demonstration, I merely marked it down as an obvious consequence of the other two; and I had scarcely sent away that paper, when on turning over the second volume of Pro-

* This error is of no trifling nature, since by it a ship may appear to change her latitude sixteen miles at the end of twelve hours, although she do not stir out of the spot, and that too in any part of the globe.

fessor Leslie's Mathematics, about to be published, I found the property which has been so "long known;" and of course perceived that I had laid myself open to an accusation so often chargeable on correspondents to periodical mathematical works; viz. the obliging the public with articles that are not new. However, as this long-known property is immediately deduced from the other two without any additional harangue, I hope I shall obtain pardon from the reasonable part of your readers for the commission of such a weighty offence.

My investigation of the other two properties has still a decided advantage over that proposed by your correspondent; in that it is founded on properties of the ellipsis to be found in every treatise, and familiar to such as know very little of Conic Sections. This end I had in view, in giving it the present form.

My remarks, however, on periodical mathematical works were not, as he supposes, so much aimed at such works themselves; nor yet at their editors, who, I well know, have enough to mind besides; but my strictures were chiefly intended for the proposers of useless puzzling questions so very detrimental to the reputation of such works, and so much complained of by many who are not at the trouble of doing so publicly.

I readily agree that every encouragement ought to be afforded to periodical mathematical works, as of the utmost advantage to the progress of science; while on the other hand, useless puzzling questions cannot be too much condemned.

Before proceeding, however, to solve his questions, I would beg to ask, Why he has allotted me the four last, rather than any of the others. Was it, because one of them (the 12th) affords a most striking instance of the truth of my former remarks, and of the little consideration with which such questions are too often proposed? The fact is, that question is altogether absurd and impossible, so long as the nature and diameter of the cylinder as well as the exact dimensions of the table-land are not given. Any one might see this at once who has ever heard of the law of gravitation—"a discovery of the great Newton which he did not owe to reading."

This therefore I consider a complete solution to the 12th question, and hope the Editor of the Ladies' Diary will have the goodness to insert it for the amusement of the ladies.

H. M.

LXX. *Memoir on a new and certain Method of ascertaining the Figure of the Earth by means of Occultations of the fixed Stars.* By A. CAGNOLI. *With Notes and an Appendix by FRANCIS BAILY.*

[Concluded from p. 360.]

APPENDIX.

SINCE the original publication of the preceding Memoir, I am not aware that any attempts have been made to derive any practical result from the theory of the learned author; notwithstanding the simplicity of the method which he has proposed. Perhaps no practicable mode, which has been hitherto suggested for determining the true figure of the earth, or the precise quantity of the compression of the polar axis, is entirely free from errors: but, as those errors probably arise from different sources, according to the methods adopted, it is desirable that the modes of investigating it should be varied as much as possible, in order that the existing discordances may be ultimately reconciled or removed.

M. Lalande has stated (*Bibliographie Astronomique*, page 613) that in the Ephemeris of Vienna for 1791 there is “a dissertation on the figure of the earth by M. Triesnecker; who has deduced, from sixteen occultations, the compression = $\frac{1}{346}$.” Whether the occultations, which were used by M. Triesnecker in his calculations, were the peculiar sort of occultations alluded to by M. Cagnoli, I am unable to ascertain; as I have not been able to procure a sight of M. Triesnecker’s labours on this subject: but, as that work was published two years before this Memoir, I much doubt whether his method is precisely the same as that laid down by M. Cagnoli. However this may be, the design of the present translation is not at all affected thereby; as my object is to recall the attention of the public to the subject, in order that the benefit and advantage of the method may not be wholly lost; and that such scientific persons, as may be induced to co-operate therein, may look out for those peculiar occultations which are described in the preceding pages, and note down their observations thereon accordingly.

For this purpose, nothing more is requisite than a telescope sufficiently powerful to see the star distinctly, when close to the illuminated side of the moon’s disc; together with a good clock, or watch, beating seconds. And although it would be desirable, in all observations of this kind, to have the *exact mean time* of the immersion and emersion of the star, yet, as it is the *duration* only from which the consequences are to be deduced, it will be sufficient (the latitude and longitude of the place being well ascertained)

certained) if the clock or watch can be depended upon for the few minutes that the star is hid behind the moon: since the *duration* will be the same whether the clock is set to mean time, or not*.

In observing occultations, similar to those which have been the subject of the preceding Memoir, the observer should be careful not to withdraw his eye from the telescope before he is fully satisfied that the star is completely hid by the body of the moon. For the star passes behind so small a segment of the moon's disc, that it may undergo a *partial* occultation by the projecting mountains of the moon, before it is *wholly* hid by that body; as appears from the following singular phenomenon, noticed by M. Koch at Dantzic, when observing the occultation of *Aldebaran* on March 7, 1794. "He was looking out for the immersion of the star, near the upper crescent of the moon. It disappeared at first: but 10'' afterwards it re-appeared suddenly in all its brilliancy. It was soon afterwards hid a *second time*. It re-appeared however again: but, presently after, its immersion took place for the *third time* at 8^h 28' 21'' apparent time. The observer then counted 30''; and, finding that the star did not make its appearance again, quitted the telescope in order to write down the observation. He came back immediately; but the star had already emerged from behind the moon. It is to be regretted that he was not present at the instant of emersion, in order to complete an observation which had never been made before. However, it was sufficient to show that the star did not pass a single *second* of a degree within the moon's disc. For, the semidiameter of the moon being 15'. 52'', and its apparent motion 29'' in a minute, it is found that, if the duration of the occultation had been *one minute*, the line joining the centre of the moon and the middle of the chord (traversed by the star) ought to be 15'. 51''." *Connaissance des Temps, Année vi.* page 253. The writer of that article justly adds that "this is one of the very interesting but very rare cases that M. Cagnoli has proposed to select for the purpose of determining the true figure of the earth."

It is not necessary to inform the practical astronomer that in many occultations the star, immediately before its immersion, and immediately after its emersion, is observed to change its colour †, and

* In calculating the exact time of the immersion or emersion of a star, in order to deduce any practical and very accurate results therefrom, I do not find that any allowance is ever directed to be made for the time that *light* takes to travel from the moon to the earth; which is about *one second and a quarter*.

† *Messier*, who observed the occultation of *Aldebaran* on Sept. 25, 1755, states that just before the immersion, on the illuminated side of the moon's disc, he saw the star sensibly diminish in light, change its colour and be-

and for many seconds to appear not only adhering to the circumference, but oftentimes projected *on the disc*, of the moon. Whether this optical deception is caused by an atmosphere surrounding the moon, or by the instrument used for the observation*, or by what other means, I shall not stop to discuss: but, as it is probable that its effects may be more discernible in those peculiar occultations which have been the subject of the preceding Memoir, it is desirable that the particular circumstances of the case in each occultation should be noted down as they occur; together with a description of the telescope used for the observation. Astronomers indeed now adopt the method of noting down the moment of apparent *contact*, as well as the moment of the *disappearance* of the star: and when it emerges again from behind the moon, they likewise note down not only the moment of its *reappearance*, but also the moment of its *separation* from the moon's disc. Indeed every remarkable phenomenon which may occur during *any* observation ought to be carefully registered: as it is only by a comparison of such cases that we can ultimately expect to discover the causes which influence or produce them.

M. Cagnoli, in his Memoir, seems to have addressed his remarks chiefly to the principal *observatories* of Europe. But, however desirable it may be to have the co-operation of those scientific bodies, it is evident that many private individuals may do *much* towards accomplishing the object and design of the author. In many cases, they may perhaps be able to do *more* than can be effected in any fixed observatory: since they may be more favourably situated, on the earth's surface, for the observation, or may enjoy the advantage of a more clear and favourable atmosphere. Indeed, the greater the number of observers attached to this duty, the greater will be the chance of an accurate result: for, independent of taking a mean of the observations made near the same spot, we might be enabled to detect the errors of the lunar tables by means of simultaneous observations made in a favourable situation in a distant part of the world; agreeably to

come *white*: and that for $10''$ before its actual immersion it appeared adhering to the border of the moon. *Connaissance des Temps, Année 1810*, page 335. In a subsequent occultation of the same star, on July 11, 1757, he states that, just before its immersion, also on the illuminated side of the moon's disc, it appeared adhering to the border of the moon for $7''$, and afterwards remained $2''$ on the moon's disc, previous to its total disappearance: and that the star was easily discernible on the face of the moon by a *reddish* colour which it had acquired on approaching thereto. *Ibid.* page 339.

* In the occultation of *Aldebaran* on July 5, 1755, *Messier*, with a $4\frac{1}{2}$ feet Newtonian reflector magnifying 140 times, saw the star for near $2''$ on the moon's disc: *Le Gentil*, with a refracting telescope of 6 or 7 feet, saw it *adhering to the border* of the moon, for $4''$ or $5''$: whilst *Cassini*, with his 18 feet telescope, saw it *detached* from the border, to the very moment of immersion. *Ibid.* page 335.

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what the author has suggested in § 15 of his Memoir. In fact, by increasing the number of observers to an indefinite extent, over the whole surface of the globe, almost *every* occultation that occurs might be brought in aid of the method proposed by M. Cagnoli.

In the present contracted state, however, of the science of Astronomy, we must confine our views to a more limited scale of assistance. In this country, indeed, there are many persons scattered throughout the kingdom who have the means and opportunity of making observations of this kind: and, amongst the numerous occultations which they observe, if they should find only *one* that belongs to that peculiar class of occultations, which is alluded to in the preceding pages, it would be a valuable discovery; and ought to be noted down accordingly, with all the circumstances attending it, in order that it may be compared with others of a similar kind.

It is much to be lamented, however, that in this country there is no association of scientific persons formed for the encouragement and improvement of Astronomy. In almost all the other branches of the arts and sciences, institutions have been formed for the purpose of promoting and diffusing a general knowledge of those particular subjects; such as Botany, Agriculture, Chemistry, Anatomy, Geology, &c. &c.: the beneficial effects of which are too evident to be insisted upon in this place. But, Astronomy, the most interesting and sublime of all the sciences (and, to our country, certainly the most useful) cannot claim the fostering aid of any society. It is however well known that many individuals, in various parts of the kingdom, have of late years erected and furnished private observatories at a very considerable expense. Nevertheless the utility of those establishments must be greatly circumscribed through the want of some mode of general communication amongst observers, by means of which their labours might be collected and registered; and thus rendered permanently useful. The formation of an ASTRONOMICAL SOCIETY would not only afford this advantage, but would in other respects be attended with the most beneficial consequences. It would induce many intelligent persons (who are at present restrained by want of due encouragement) to come forward as the patrons and followers of the science; and excite the more experienced to further exertions and new discoveries*. Although

* The name of the *Royal Society* will naturally occur to the reader on this occasion: but that society was formed for the promotion and encouragement of science *in general*; and the subject of Astronomy appears to form but a small portion of its labours. Nevertheless the *Astronomical Society* would, in common with the *Linnean*, *Geological* and other scientific societies, hope for the *co-operation* of its learned members.

much has been already done towards perfecting our present system of astronomy, much still remains to be effected, both in theory and practice. Not only would the interchange of sentiments and remarks on the various celestial phænomena, which such a Society would afford, lead to the advancement of the science in general; but the comparison and discussion of the merits of the several instruments used for astronomical purposes would also tend either to their improvement or to the discovery of new ones. By means also of a society of this kind an active communication might be kept up with the principal astronomers in different parts of the world; and, thus, continual subjects of research or observation be submitted to public attention. The funds of such a society might likewise be usefully employed in the formation of an *Astronomical Library*, consisting of the most rare and valuable books on the science; and in collecting and transcribing the observations of astronomers in various parts of the world*. But even without any views or pretensions to new discoveries, or to the improvement of optical instruments and time-keepers, or to the formation of such a library, an Astronomical Society might render very important benefits to Geography and Navigation by simultaneous observations of lunar distances, eclipses, occultations, and various other phænomena; which are at present in a great measure neglected because the public attention is not specially directed thereto. And the more numerous and widely diffused the members of such a society might be, the greater the probability of obviating the baneful effects of our cloudy atmosphere; an occurrence which often renders the efforts of the finest observatory totally unavailing.

There is yet another source of assistance which we may hope

* The observations and remarks of many eminent astronomers, both ancient and modern, still exist only in manuscript, in the hands of private individuals; and ought, if possible, to be rescued from their present precarious situation, and deposited in a place of safety and convenient reference. *Buliald* made an immense collection of observations of this kind, many of which came into the possession of *Lemonnier*; who put them into the hands of *Pingré*, for the purpose of publication. *Pingré* had himself collected also an astonishing number of rare and valuable manuscripts from all parts of Europe, and had not only calculated and reduced their contents; but had likewise discussed and compared the observations, so as to render it a work of the highest utility to astronomers. It was the labour of thirty years! and the National Assembly in 1791 issued a decree for printing it, under the title of *Annales Célestes du dix-septième siècle*. In the year 1794, about 360 pages of this work were printed; being one-third only of the intended publication. The learned author died in 1796 at the advanced age of 85: since which period I do not find that the progress of the work has been at all advanced. Many valuable observations of the more recent astronomers, such as *Halley*, *De L'Isle*, *Lemonnier*, *Godin* and others, it is well known, exist also only in manuscript; and are now mouldering in the dust, a disgrace to the nineteenth century.

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for in this country. It is well known that, on the continent, great advantage is rendered to astronomy by means of the *Ephemerides*, which are published under the authority of Government: and those annual publications contain a vast fund of valuable information on the subject of astronomy, which otherwise might be for ever lost to the world. The Board of Longitude in this country have *now* the power and the means* of affording similar assistance by enlarging the original plan and design of the *Nautical Almanac*, and by assimilating it to those which are published at Paris, Berlin, Vienna, and other places: a measure which would tend to retrieve the character of the work, and redound to the honour of the country†.

With the very ample funds (4000*l.* per annum) which Government has placed at the disposal of this new Board, for scientific purposes, united to the known abilities and zeal of its several members, we may confidently hope that a new impulse will soon be

* Extract from the recent Act, 58 Geo. III. cap. 20, § 6. "And whereas it is expedient that the said Commissioners should be enabled to expend certain sums towards making experiments of instruments, modes or proposals, and for making and publishing observations, calculations, and tables for ascertaining the longitude, or towards improving or correcting such as may have been already made, or *for other purposes useful to navigation*; be it enacted that they may pay or expend any sum or sums of money, not exceeding 1000*l.* in any one year, towards the making, correcting or publishing any such experiments, modes, observations, calculations, or tables.

§ 7. "And whereas it is expedient that the said Commissioners should be enabled to cause to be ascertained, as accurately as may be, the latitude and longitude of places, whereof the exact situation hath not been already sufficiently ascertained; be it enacted that they may expend or cause to be expended any sum, not exceeding in the whole 1000*l.* in any one year, for that purpose.

§ 8. "And whereas it may happen that proposals, inventions and tables, or corrections and amendments of former proposals, inventions or tables, ingenious in themselves and *useful to science*, and which may *deserve encouragement*, (though they do not come within the limits and conditions specified for the above-mentioned rewards) may be made to the said Commissioners; and it is expedient that they should be enabled to bestow such moderate rewards upon the person or persons who may have made such proposal, invention, or correction; be it therefore enacted that the said Commissioners may pay or cause to be paid such sum, not exceeding 500*l.* to any one person for any one proposal or invention, or 2000*l.* in one year, as they may consider the said proposals, inventions, tables or corrections to deserve."

† Amongst the several amendments which might be made to that useful work, I would suggest the propriety and advantage of having the Right Ascension and Declination of the moon inserted to *seconds* of a degree; similar to the method *now* adopted in the *Connaissance des Temps*. This would facilitate the finding of the apparent place of the moon, for the purpose of comparing it with the place of any given star, the position of which is always given in Right Ascension and Declination: and would moreover obviate

be given to the progress of astronomy in this country: and that, by emulating our scientific neighbours on the continent, we may avoid the lamentable necessity of resorting to them for almost all our means of deducing the situation of the heavenly bodies, and of investigating the laws which govern their motions *. Probably the object of the Legislature would be more effectually answered if the new Board of Longitude would undertake to *lead and direct* the attention of the scientific world to such particular objects as they might think most worthy of encouragement; rather than to *wait for proposals* on every subject (useful or visionary) that may be laid before them. The *publication* of specific rewards—for the formation of new Tables of the Sun, Moon, or any of the Planets†;—for the best Essays on any particular subject of Practical or Theoretical astronomy;—for any valuable improvement in Time-pieces, Telescopes, Micrometers, or other astronomical instruments;—for the best Engravings of any portion of the celestial sphere, and particularly of the Zodiacal stars‡;—for reducing the Observations of any celebrated astronomer;—for the Discovery of any New planet, comet, fixed star, or other remarkable celestial phenomenon;—for the Translation of any valuable astronomical treatise into the English language;—and, in general, for any other object which may be “*useful to science and which may deserve encouragement*,”—would necessarily excite and fix the public attention to those subjects, and perhaps more effectually promote the views of a liberal and enlightened Government.

But to return to the subject of the present Memoir.—It has been stated by M. Cagnoli, § 4, that the difference of parallax

viate the necessity of finding the position of the *Nonagesimal*,—an unnecessary and troublesome process. As the original calculations of the computer must extend to *seconds*, there is no good reason for omitting them in the publication. I would likewise (as connected with the subject of this Memoir) take the liberty of suggesting the propriety of publishing annually in that work, an accurate list of all the Zodiacal stars (including even those of the 9th magnitude) not only in Right Ascension and Declination, but also in Longitude and Latitude, with their annual and secular variations, corrected from the latest observations. The expense would be trifling, compared with the advantage to be derived from the result.

* It is too well known that *all* our astronomical Tables, *all* our Catalogues of stars, and *most* of our astronomical Formulæ, in present use, are the production of the continent.

† I believe there are no tables whatever of two of the newly discovered planets.

‡ Accurate engravings of the Zodiacal stars, upon a large scale, would enable astronomers with greater certainty to look out for occultations of fixed stars by the moon; the observations of which are undoubtedly the best method of determining the longitude of places.

arises

arises from the inequality of the terrestrial radii; and that the one is the measure or consequence of the other. Therefore, in order to determine the parallax at any given point of the earth's surface, it is necessary to know *the distance of that point from the centre of the earth*. But, that the reader may see the anomalies which arise on this subject, (from the actual measurement of the degrees of the meridian in different countries, and from different theories which have been assumed) I have calculated the following table, which shows the radius of the earth in the latitude of Greenwich ($51^{\circ}.28'.40''$), according to the several hypotheses of the compression of the earth's axis therein stated.

No.	Hypothesis.	Radius in feet.	Difference.
1	$\frac{1}{139}$	20 827 239	
2	$\frac{1}{130}$	20 833 848	6609
3	$\frac{1}{120}$	20 848 224	14376
4	$\frac{1}{200}$	20 855 339	7115
5	$\frac{1}{230}$	20 863 689	8350
6	$\frac{1}{250}$	20 868 144	4455
7	$\frac{1}{300}$	20 876 819	8675
8	$\frac{1}{309}$	20 877 920	1101
9	$\frac{1}{330}$	20 880 560	2640

The first value here given arises from the recent measurement of the arc in France, by taking the middle arc at Evaux, which makes the compression of the earth's axis $= \frac{1}{139}$; and the length of the earth's radius in the latitude of Greenwich equal to 20 827 239 feet. By comparing this value with the last in the table, which is one of the hypotheses assumed by the *Bureau des Longitudes* in the recent tables of the Moon by M. Burg, it will be found that there is a difference of no less than 53 321 feet, or more than 10 miles; a quantity sufficiently great to be detected by the observation of the moon's parallax, and which would produce a very sensible effect on the duration of such occultations as have been the subject of the preceding Memoir: since we have seen that, in some cases, a difference of less than 200 feet will produce a difference of one second in the duration.

But, although such occultations may clearly show that the earth is compressed at the poles, and although they may be effected by quantities which differ so widely from each other as in the case just mentioned; yet it may be doubted whether they are capable of determining the *precise quantity* of that compression; and therefore

therefore whether they are more eligible for that purpose than the methods at present in use. In reply to which, it may be repeated that this plan is at present proposed only *in aid* of the modes hitherto adopted for determining the true figure of the earth; and by no means as supplying the place of other methods for deciding that difficult problem.

In order, however, that the reader may have a more comprehensive view of this subject, and be enabled to see the differences which would arise in various latitudes and under various circumstances, according to two nearly similar hypotheses of the earth's compression compared with the hypothesis of its being a perfect sphere, I have inserted the three following tables; which show the alteration in the position of the star within the moon's disc, and its consequent effect on the duration of the occultation, according to the three several hypotheses of the earth being considered as a perfect sphere, and of its being compressed at the poles $\frac{1}{300}$, and $\frac{1}{330}$; at the several heights of 10° , 20° and 30° of the moon above the horizon; and at the several latitudes of 50° , 60° , and 70° . The chord of the star's apparent path behind the moon being, in all these cases, assumed to be perpendicular to the vertical circle of the place.

Thus, by an inspection of Table II, it will be seen that in latitude 60° , when the moon is 10° high, and the star $60''$ within the moon's disc, the duration of the occultation would be $20'.7''$, if the earth were a perfect sphere. But, if the polar axis were compressed $\frac{1}{300}$, the star would then be only $51''.5$ within the moon's disc, and the duration of the occultation only $18'.41''$: which is the case alluded to by M. Cagnoli § 10. Or, if the polar axis were compressed only $\frac{1}{330}$, the star would then be $52''.3$ within the moon's disc, and the duration of the occultation would be $18'.50''$; being a difference of $9''$ between the results of the two hypotheses. This *difference* would be the same, if the moon were 20° high; as may readily be seen by an inspection of the same table: it would also be the same under corresponding circumstances, if the observation were made in latitude 70° : as may be seen by an inspection of Table III.

This difference however may probably be considered as too small to enable us to derive any satisfactory result as to the *precise quantity* of the compression of the earth's axis. But, if we take the position of the star within the moon's disc equal to $10''$ only, and the moon at the height of 10° as before, we shall have the duration of the occultation $8'.19''$ on the supposition that the earth is a perfect sphere. Then, supposing the axis to be compressed $\frac{1}{330}$, the star would be only $2''.3$ within the moon's disc, and the duration of the occultation only $4'.0''$: or, supposing the axis to be compressed $\frac{1}{300}$, the star would be only

1",5 within the moon's disc, and the duration only 3'. 14"; being a difference of 46" in the result of the two hypotheses. If the moon were 20° high, the difference would be only 42". If however the observation were made in latitude 70°, and the moon at 10° high, the difference would be much greater: since, on the supposition of the compression = $\frac{1}{330}$, the duration would be 2'. 30"; whereas, on the supposition of the compression = $\frac{1}{300}$ the star would not undergo any occultation at all, but would merely *touch* the moon's disc. Nearly similar results might be obtained, under particular circumstances, in lower latitudes; as may be seen from an inspection of the Tables. And it will thus be evident that a *very small* difference in the ellipticity of the earth, may, under some very favourable circumstances, be rendered sensible to observation.

It is unnecessary however to dwell further upon this subject at present: a fair experiment does not appear to have ever yet been made of the ingenious mode proposed by the illustrious author of the Memoir, for determining the true figure of the earth. It seems to me to be highly deserving of encouragement: and my object will be fully answered if any practical good should result from the public attention which I may have excited in its favour.

F. B.

TABLE I.

Latitude 50°.						
Height of the Moon.	Earth = Sphere.		Earth = $\frac{1}{330}$		Earth = $\frac{1}{300}$	
	* within D's disc.	duration.	* within D's disc.	duration.	* within D's disc.	duration.
10°	60	20. 7	54,0	19. 7	53,4	19. 1
	30	14.20	24,0	12.51	23,4	12.41
	15	10.11	9,0	7.54	8,4	7.38
	10	8.19	4,0	5.16	3,4	4.52
	6,6	6.46	0,6	2. 2	0	Appulse.
20°	60	20. 7	54,3	19.10	53,7	19. 4
	30	14.20	24,3	12.56	23,7	12.46
	15	10.11	9,3	8. 2	8,7	7.46
	10	8.19	4,3	5.28	3,7	5. 4
	6,3	6.37	0,6	2. 2	0	Appulse.
30°	60	20. 7	54,7	19.14	54,2	19. 9
	30	14.20	24,7	13. 2	24,2	12.54
	15	10.11	9,7	8.12	9,2	7.59
	10	8.19	4,7	5.43	4,2	5.24
	5,8	6.21	0,5	1.52	0	Appulse.

TABLE

TABLE II.

Latitude 60°.						
Height of the Moon.	Earth = Sphere.		Earth = $3\frac{1}{3}$ '.		Earth = $3\frac{1}{10}$ '.	
	* within D's disc.	duration.	* within D's disc.	duration.	* within D's disc.	duration.
10°	60"	20. 7	52,3	18.50	51,5	18.41
	30	14.20	22,3	12.23	21,5	12.10
	15	10.11	7,3	7. 7	6,5	6.43
	10	8.19	2,3	4. 0	1,5	3.14
	8,5	7.41	0,8	2.22	0	Appulse.
20°	60"	20. 7	52,7	18.54	51,9	18.45
	30	14.20	22,7	12.30	21,9	12.17
	15	10.11	7,7	7.19	6,9	6.55
	10	8.19	2,7	4.20	1,9	3.38
	8,1	7.30	0,8	2.22	0	Appulse.
30°	60"	20. 7	53,2	18.59	52,6	18.52
	30	14.20	23,2	12.38	22,6	12.29
	15	10.11	8,2	7.33	7,6	7.16
	10	8.19	3,2	4.43	2,6	4.15
	7,4	7.10	0,6	2. 2	0	Appulse.

TABLE III.

Latitude 70°.						
10°	60"	20. 7	50,9	18.54	50	18. 2
	30	14.20	20,9	12. 0	20	11.44
	15	10.11	5,9	6.24	5	5.54
	10	8.19	0,9	2.30	0	Appulse.
20°	60"	20. 7	51,3	18.39	50,5	18.30
	30	14.20	21,3	12. 7	20,5	11.53
	15	10.11	6,3	6.37	5,5	6.11
	10	8.19	1,3	3. 0	0,5	1.52
	9,5	8. 7	0,8	2.22	0	Appulse.
30°	60"	20. 7	52	18.46	51,2	18.38
	30	14.20	22	12.18	21,2	12. 5
	15	10.11	7	6.58	6,2	6.34
	10	8.19	2	3.44	1,2	2.54
	8,8	7.49	0,8	2.22	0	Appulse.

LXXI. *Report from the Select Committee appointed to consider the Validity of the Doctrine of Contagion in the Plague.*

The Select Committee appointed to consider the validity of the doctrine of Contagion in the Plague; and to report their Observations thereupon, together with the Minutes of the Evidence taken before them, to the House;—HAVE considered the matters to them referred, and have agreed upon the following REPORT:

YOUR Committee being appointed to consider the validity of the received doctrines concerning the nature of contagious and infectious diseases, as distinguished from other epidemics, have proceeded to examine a number of medical gentlemen, whose practical experience or general knowledge of the subject appeared to your Committee most likely to furnish the means of acquiring the most satisfactory information. They have also had the evidence of a number of persons whose residence in infected countries, or whose commercial or official employments, enabled them to communicate information as to facts, and on the principle and efficacy of the laws of quarantine. All the opinions of the medical men whom your Committee have examined, with the exception of two, are in favour of the received doctrine, that the plague is a disease communicable by contact only, and different in that respect from epidemic fever; nor do your Committee see any thing in the rest of the evidence they have collected, which would induce them to dissent from that opinion. It appears from some of the evidence, that the extension and virulence of the disorder is considerably modified by atmospheric influence; and a doubt has prevailed, whether under any circumstance the disease could be received and propagated in the climate of Britain. No fact whatever has been stated to show, that any instance of the disorder has occurred, or that it has ever been known to have been brought into the lazarettos, for many years. But your Committee do not think themselves warranted to infer from thence, that the disease cannot exist in England; because, in the first place, a disease resembling in most respects the plague, is well known to have prevailed here in many periods of our history, particularly in 1665–6; and further, it appears that in many places, and in climates of various nature, the plague has prevailed after intervals of very considerable duration.

Your Committee would also observe, down to the year 1800, regulations were adopted, which must have had the effect of preventing goods infected with the plague from being shipped directly for Britain; and they abstain from giving any opinion on the nature and application of the quarantine regulations, as not falling

within the scope of inquiry to which they have been directed ; but they see no reason to question the validity of the principles on which such regulations appear to have been adopted.

June 14, 1819.

MINUTES OF EVIDENCE.

Charles MacLean, M.D.—Employed himself in 1815 in investigating the plague in the Greek Pest Hospital near the Seven Towers at Constantinople, commonly called “The Plague of the Levant:” Considers the plague as not contagious. Never knew an instance of a plague case imported into England—has reason to believe such an occurrence has never happened during the existence of quarantine or before.—Does not consider the plague of 1665 to have been the Levant plague. Is of opinion that the plague is capable of being cured in the proportion of four cases out of five by particular treatment.—Has heard of inoculation for the plague—of a gentleman (Dr. White, who died in Egypt) inoculating himself three times and not taking the disease the two first times ; but being seized the third time with the malady in consequence of a coincidence which in Dr. M.’s opinion would have equally happened whether he had been inoculated or not. Knows it to have been Dr. White’s opinion that the plague was not contagious. Considers the establishment of our own quarantine laws not to be of the smallest use—that is, if it be true that there never has arrived any person from the Levant, or any other place, actually labouring under the plague ; and if it be true (according to the advocates for contagion) that goods, wares, and merchandize can retain infection for seven, fourteen, or twenty years, it must be apparent that, with respect to goods as well as with respect to persons, a quarantine of forty days can in such case be of no sort of use. Knows that the regulations of the Lazaretto system adopted in Italy have not been effectual for preventing contagion. Recently at the town of Noya, which was surrounded by lines of circumvallation, ditches, and cordons of troops, and every mode of restriction imposed on the inhabitants, the disease continued, as it has done at other places subject to plague police, its usual course, and ended at the usual time. Persons employed as expurgators of goods at the Lazaretto are more exempt from the disease than the community at large. Feels assured that from mere contact he could not take the disease. Does not believe, as has been represented, that in the towns of the East the Turks suffer in a greater proportion than the Christian population. The Turks do not desert their friends when seized with the disease, not feeling that dread of the malady which Christians do ; and it is admitted by the advocates of contagion, that dread operates more severely than what they call the true contagion

contagion itself; it therefore must operate equally severely under the belief of contagion, whether it does or does not exist. And it must operate more severely upon those who entertain that belief, as the Greeks and Armenians of the Levant, than upon the Turks, who do not entertain it. This also has been confirmed by facts; and it is stated by various travellers, that the Turks recover in a much greater number from the plague than the Christians who are attacked by it. Considers the date of the Council of Trent to have been the period at which a belief of contagion in epidemic diseases was first accredited and acted upon by any public authority, with the view then of effecting the removal of that Council to Bologna. It was disputed by the Cardinals whether it was the plague or scarlet fever. Fracastorius said it was the true plague. Has no doubt that the disease which prevailed in Florence in 1348 was the true plague.—[Being asked to state more particularly the grounds on which he rests his opinion that the plague is not contagious,]—Thinks the plague not contagious for many reasons. In the first place, the plague and all other epidemic diseases appear at certain periods, generally speaking, and disappear at other certain periods, different in different countries; they also cease generally at the time at which the greatest number of persons are affected, as happened in the plague of London in 1665, in the plague of Marseilles in 1720, and it is believed in most other pestilences—facts which seem wholly incompatible with the existence of contagion. Besides, they are capable of affecting the same persons repeatedly, which there is no proof that contagious general diseases are capable of doing. Conceives the principal causes of epidemic diseases to be—the epidemic constitution of the air, sudden or extreme vicissitudes of temperature, deficiency of nourishment, and depression of mind. The earliest epidemic season in the year is at Smyrna, where it occurs from February and March to June or July; considered generally to terminate about the 24th of June. At Constantinople it commences in July or August, and terminates in November or December. It commences at Cairo much about the same time as at Smyrna; seems to correspond with the rising and falling of the Nile. The epidemic season does not appear at all connected with the change from heat to cold, except in as far as sudden transitions are concerned; it commences in England and other countries in hot weather, and terminates in cold; in Egypt and Syria it commences in cold weather, and terminates in hot. Conceives the clothes sold out of the Pest-hospitals could not fail to produce the plague, if the plague were contagious. It is customary for the relations of those who die of the plague in Turkey, to wear the clothes of the deceased, or to sell them

in the public bazaar or market-place ; they are in constant circulation. Used to walk into the city of Constantinople every day, sometimes even after he had the disease, and go through the thickest of the people along with his interpreter, visiting the coffee-houses and other frequented places. The people knew they were making experiments in the plague hospital, and none of the Mahometans ever avoided them on that account, nor was the disease by that means propagated. The purveyor and other agents of the hospital walked every day to the open market to buy their supply of victuals for the hospital ; they came openly among the people without any precaution.

Thomas Foster, M.D..—Conceives the plague under certain circumstances is contagious ; for instance, wherever there is close confinement in a chamber in which atmospheric air is not freely admitted ; but if atmospheric air be freely admitted into the chamber of the patient, the attendant will be, generally speaking, free from contagion. Considers that we have had no cases of the Levant plague in England. Does not think the plague of 1665 was the Levant plague. Could never find any evidence of a plague case existing any where in England. Believes that contagious diseases can attack persons more than once. Considers contagious diseases such as are capable of being communicated by contact and inoculation ; infectious diseases, such as arise from the infecting state of the atmosphere. Has considered the quarantine establishments as they related to the medical question, in what manner the plague is capable of being communicated ; and the result of his inquiries has been satisfactory to himself, that the free admission of atmospheric air into chambers was, in general, a preventive against the propagation of the disease. Thinks that if bale goods be capable of receiving the infection in the Levant, so as to convey it all the way to London, the short time limited for quarantine would be insufficient to prevent the danger ; that the cause of pestilential diseases consists in the infectious qualities of the air, which are capable of exciting the disease on predisposed constitutions ; that those peculiar qualities of the air operate in some instances locally and continually in particular regions : moreover, that unhealthy qualities of the air occur in all places casually, and excite prevailing epidemics and influenzas in particular seasons, which the predisposed soonest fall a prey to.

Dr. James Johnson.—Has not personally seen the plague, but has served in the Mediterranean, where he has had opportunities of acquiring information. Has not the least doubt of its being contagious through the medium of contact, near approximation, or exhalation, and fomites. Considers articles of cargo not

so likely to be infected as clothes ; considers packages not to be near so likely to transmit or bring contagion, as articles of dirty apparel. In woollen clothes the fomites of contagions is more generally retained than in any other substances.—Supposing one of the most likely articles to communicate the plague to be infected, such as wool, silk, or cotton ; considers that in order to purify those articles, nothing more is necessary than ventilation ; free ventilation, and opening them out to the action of the sun and air. Thinks it is extremely seldom that contagion is brought in goods ; but that the purification is more frequently a process of safe precaution than of actual dispersion of the contagion. Considers the quarantine laws too rigid ; the time is too long, and probably the process too complicated. Would not consider the fact of there not having occurred a plague case at any lazaretto for 50 or 100 years, sufficient to inspire confidence to contemplate the doing away quarantine establishments, because he knows that the contagion of plague is very considerably under atmospheric influence ; and consequently, that a very long period may occur in which that peculiar constitution of the air is absent which gives activity to the matter of contagion. Adduces as particular circumstances in proof that the plague is contagious or infectious ; First, the authority of those who have written without bias on the subject ; for instance Russel, and most of those who have seen it. Secondly, its being an *eruptive* disease ; because we know that all eruptive diseases arise from specific contagions, or poisons as they are called ; buboes and carbuncles are as commonly seen in the plague, as pustules in the small-pox. Considers the malignant fevers incident to Trincomalee, Batavia, and Diamond Harbour, to be a class totally different to the plague, as it exists in Turkey, Egypt, and in the African states, both in their nature and causes. Believes these violent epidemics in India depend principally for their origin upon the miasmata exhaling from the marshy soils of these regions, which miasmata are influenced by the state of the atmosphere, and that the diseases are not in their own nature contagious ; but that under particular states, as from accumulation of filth and want of ventilation, they do occasionally assume a contagious character. Considers that *all fevers*, whether originally contagious or not, may become so by the patients being too much crowded, by want of cleanliness, or want of ventilation. Believes the remittent and what are called Yellow fevers, more properly called Endemic fevers, are not contagious. Was at Gibraltar in 1800 ; the fever was not epidemic then ; there were sporadic cases, which he considered as of local origin ; they were produced by causes generated in the surface of the rock, and atmospheric causes—not imported. By the term sporadic means wandering cases, not generally epidemic ; a case

happening here and there in a family. Believes the plague to be a contagious disease communicable by the effluvia of diseased bodies being applied to sound persons, independent of any atmospheric or adventitious causes.

Being asked whether he conceives that a certain quality in the air is necessary to bring forth latent contagion ; that in the great plague in the reign of Edw. III. which spread over country after country, and carried off three-fourths of the inhabitants of Europe, the climates shifted : if from the year 1570 to 1665, there was a perpetual recurrence of plague in England, it is probable that the same quality of the air could have been over England during a century ?—answers, “ With respect to the first question, the disease was probably produced by an epidemic influence, which will occasionally travel round the whole globe ; for instance, in 1802 there was an influenza over most of the world. There was a fever in India a few years ago, which travelled nearly 1000 miles, gradually extending itself in the direction of the monsoon, from near Cape Cormorin to the banks of the Carvery, and sweeping off 106,000 people. This took a considerable time to travel from one part to another ; but went in the direction of the monsoon, affecting one district after another. That I consider as an epidemic influence which I cannot account for, excepting by peculiar states of earth and air ; it was not contagious from individual to individual, but something in the air, which produced a general epidemic fever from south to north. I think it was some epidemic influence of this kind that spread over Europe ; but from the time it happened, the descriptions are not minute ; I hardly consider it the plague of the present day.” Considers that there are periodical changes of climate, irregular in their returns, but bringing a constitution somewhat similar to former periods. This was the opinion of Sydenham, and is entertained by many at this moment. Thinks these changes productive of disorders, and that there are scarcely two epidemics precisely alike in their nature. Believes the plague of 1665 to have been the regular true Levant plague. It appears that the plague was, at least, frequently recurrent in England for the greater part of a century, previous to 1665 ; in the year 1608 it is mentioned by a familiar writer to be so prevalent, that houses were marked with a cross, and the words *miserere mihi* written on them to prevent persons from entering them ; he particularly mentions the plague *spots* ; the *tokens* were probably buboes or carbuncles ; with respect to the *spots*, there is some difficulty in making up one’s mind as to what they meant, they might mean *petechiæ*.

Dr. William Gladstone, Surgeon to the Naval Asylum at Greenwich.—Was at Constantinople in 1806 and 1807 ; and from having been then surgeon of His Majesty’s ship the *Endymion*,

mion, saw there some diseases of the plague, and a great variety of Asiatic fevers, highly infectious. Considers the plague, from what he has seen, as highly infectious, and equally so through the medium of the diseased atmosphere of a sick chamber, as by simple contact, by feeling the pulse. Supposes the plague at Constantinople to arise from the diseased constitution of the atmosphere and other peculiar causes, such as effluvia and soil, which produce endemic diseases all over the globe; from the same causes as we have epidemic diseases in England, and from the circumstance of that city standing upon hills. Many of the houses are built on ground sloping to the south-west, consequently liable to the whole action of the south-west sun; all are badly ventilated. The streets are very narrow, and they do not possess that grand source of health, common sewers. The suburb of Pera, which is chiefly inhabited by Europeans, has generally less plague than any other district; owing, he conceives, to the houses not being so close, or the streets so narrow. Does not consider the plague in England in 1665 to have been the real Levant plague. At that period, as far as he has been able to trace from a variety of old authors, there was scarcely such a thing as a common sewer. The privies were accumulated under every house, probably not emptied for years; and an order was given to empty them once a month. That order originated, it is believed, in the College of Physicians after the spreading of the plague. Ascribes the sickness of 1665 to the narrowness of the streets, accumulation of filth, and want of ventilation; and probably a diseased constitution of the atmosphere at that period. Firmly believes that it was not imported, but that it originated in England. Does not consider that our quarantine establishments have kept the plague from being introduced into Great Britain or Ireland. From having been frequently under quarantine restraint himself, has made it his business to visit most of the lazarettos between Gibraltar and Constantinople; but the source of disease is more frequently seen among the Greek vessels that carry cargoes to Marseilles. Considers the lazarettos particularly inefficient in fitment for the purpose of purifying bales of goods from infection, that is with respect to ventilation and ballast. Thinks it very doubtful whether the Levant plague can exist in a British atmosphere, but that there is great encouragement to nurse disease, if any is imported into the lazarettos. There are some of the lazaretto ships where the shingle ballast has not been shifted for many years; and in many instances fevers have been produced, and nursed from this cause, even in our men of war; the men of war formerly used to be ballasted with shingles; on turning this ballast, it has produced fever in several of the ships. Has never

heard of a plague case having arrived at or been seen in any lazaretto in Great Britain.

Being asked, What is the state of atmosphere which he conceives compatible and not compatible with the existence of plague? —answers, “ I look upon it that in cold dry weather the plague does not so frequently exist. In hot weather, after floods, when the rivers, such as the Nile, have overflowed, and left marshes and ponds, the action of the sun in summer on such marshes and moist ground always produces disease, and frequently in the Levant plague. In the cases of plague which I saw at Constantinople, the thermometer stood about the freezing point, from 26 to 30; it was in the winter.” The state of the atmosphere in which it is supposed to act most violently, is a high temperature from 66 to 76 and upwards. Thinks a lazaretto properly fitted up with ventilating apparatus, so as to cause a current of air to be constantly percolating through the cargoes, must soon destroy the vitality of any contagion that might be conveyed to England. Is of opinion, that the airing process might be as efficiently performed, as it now is, in a much shorter period, by a different fitment, attending to the state of the ballast and hold, which in every ship is important, but in lazarettos most particularly so. Believes it possible that the plague might be imported into England. Sees no reason why it should not spread, except that the English people are more cleanly, better ventilated in their apartments; and the common shores and drains carry off all filth, which is a great cause of the spreading of the plague in other countries. Does not suppose the atmosphere of England applicable to the receiving or generation of plague, for the last one hundred years. This country and every part of the world inhabited, has been more cultivated, underwood near cities has been cleared away, and swamps drained, which has contributed much to rendering the disease milder. Knows that the plague is frequent in Aleppo, and that the caravans proceed regularly with goods in bales from Aleppo eastward through the continent of Asia; but never heard of the plague being communicated by these caravans to the eastern country. Sees no reason why it should not have been so communicated as well as by goods or persons on board ship westward, except that the goods are not so closely packed in caravans as in Levant ships. In ships, the cargoes are screwed down; they often raise the beams of a ship in forcing the goods down; and consequently they are more liable, from their close stowage, to retain infection, if infection is embarked.

Dr. Augustus Bozzi Granville.—Has seen the plague in various parts of Turkey, Greece, Asia, Syria, Egypt, &c. and in Constantinople, where he resided two years, and has no doubt that

that it can be conveyed by an individual infected by it to another in perfect health. Ascribes our not having it in Great Britain to the regulations of the quarantine laws.

Being asked what precautions are taken to prevent infection, by the Frank inhabitants of Smyrna and Constantinople, and other places visited by the plague?—answers, “If they can afford it, shutting themselves up in the houses before communication with persons infected; if they are obliged to go abroad, as some are, such as physicians who have their livelihood to get, some wear oilskin dresses, oilskin gloves, and other medical precautions to prevent breathing the infected air; others anoint themselves with oil, and avoid contact as much as possible, under a strong persuasion that contact produces disease. In Egypt and Syria they shut themselves up as soon as there is a rumour of the plague, and never quit till the dews fall, that is, till St. John’s day; then they come out, and proceed to church in order to sing *Te Deum*.”—During the prevalence of great disease in any of these towns, never knew the plague destructive in the families of the Franks, who avoid contact with diseased persons. The plague is not epidemic. Can bring cases in support of the assertion, and that it does not depend on atmosphere or ventilation. The state of the air may render the person exposed to the contact, more or less liable to feel its effect, but will not operate in checking the disease. Attributes the periodical appearance of the plague in the spring and autumn to the seasons having an influence on the character of the disorder; the same as in this country, in winter, we are more likely to catch a cold or catarrh. Thinks the plague most probably an endemic disease, at some particular parts of Egypt. The first mention of it is as coming from that country. Refers to Thucydides, though of opinion that the plague of Athens mentioned by him was not the plague of the present day. The other authors who mention the disease are Muratori, Guastaldi, Foderé, Nacquart, and very recently Jourdan and Valli.

Cases of plague not so frequent in the division of Constantinople called Pera, as in Constantinople, because every Frank takes precaution against the disease. That suburb is a little more elevated, and is a long narrow street; as to the houses, many of them are of stone, whereas in Constantinople they are chiefly wood; and the streets are wider at Pera than they are at Constantinople, generally speaking. Pera upon the whole is a more airy place than Constantinople; but does not think it a less likely situation for the production of any disorder peculiar to the climate, than Constantinople. Knows that caravans proceed very frequently for the conveyance of goods from Aleppo eastward,
through

through the continent of Asia; but has never heard that the plague was conveyed by those caravans, eastward, so as to establish itself, except among some of the few thinking Christians; the mass of the people never think of the disease at all.

Damascus became affected with the plague in 1804; it was carried thither by the army of some Peishwa, who had been on the coast to assist in the reduction of Jean d'Acre. Bagdad is often, and has been lately infected with the plague. Has heard of the plague being communicated westward of Constantinople, over land to Adrianople. Believes the plague which raged there in 1812, was nearly as fatal as it proved at Constantinople. Has also continually heard of the plague being communicated from vessels from Smyrna, to many parts of the Levant.

Being asked to explain the difference between infection and contagion?—answers, “Contagion is a mere mode of action resulting from the habit of certain diseases to affect individuals; it is not a principle, such as the electric fluid and such kind, as many persons give an idea of in their writings, flying about the air. Contagion expresses this: during such a disease as the plague, there are certain animal emanations which partake of the morbid state of the body from which they issue; when these are applied by direct contact, or by any mediate contact, namely, objects on which these emanations rested, to an healthy body, it will contract the disease. Infection is this: infection is a peculiar state of the atmosphere, which has been rendered unfit for the healthy exercise of life, by the crowding together of a number of persons ill of the same fever, in a given place, and during a given time; thus an epidemic may become infectious.” There are examples, and those very authentic, proving that this matter of the plague can, if applied to an healthy body, cause the disease to break out even at a very long period after; should say several months. There is one instance in point, among the most recent, and it rests on the highest authority. During the plague at Corfu in 1815, one of the villages which had been infected several months, had for some time, I believe for 43 days, exhibited no sign of the plague, owing to the measures of segregation adopted by Sir Thomas Maitland; the village was reported to be released, and fumigation preparatory to its receiving *Pratique*, ordered; the officer who had the *surveillance* of the village during the three or four months had resided in the church, from there being no house that was not thought infected, in which church the people and the priest had been crowded just before the laws of segregation were ordered by Sir Thomas Maitland; some of these died subsequently, for the church was ordered to be shut the instant the plague began. It was therefore necessary
to

to purify the church before the people could go in again, as well as the village altogether. Leave being granted, the priest went in, and touched the cloth of the great altar, so as to shake it to purify it, when he was seized with the plague, beginning with the head-ache, so as to cause him to fall on the steps of the altar almost immediately; and in three hours, before he could be carried to the lazaretto, he expired, with buboes under the arm and livid spots over the body. Is of opinion that the contagious matter of the plague must be brought either by persons or in bales of goods on board ship from the Levant to England; and that persons touching the infected portion of merchandize packed in these bales, must exhibit such phænomena in the lazarettos in England, as now described to have happened in the village in Corfu. Cases in point have happened at the lazaretto at Leghorn since 1814; at Marseilles within fifteen years, twice; and recently, according to the dispatches of Mr. Hoppner, the British consul at Venice, in October 1818. Has never heard of the plague being caught by any of those persons appointed to see the quarantine laws put in execution in the lazarettos in England; but conceives that its non-appearance in England does not do away with the contagious nature of the disease. Knows that during the prevalence of the plague in the Levant, goods are in general not allowed to be shipped for England under the quarantine laws, till after the disease has ceased. If the length of time is very great between the time of shipping and unloading, and if certain circumstances have taken place, either on the removal of the cargo during the voyage, or in altering it, or the vessels meeting with bad weather and being washed over and over again, it is not improbable to suppose that part of the plague-matter, if any existed in the cargo or attached to any part of the vessel, may have been weakened in its virulence; but begs to give that as a supposition, and not as belief, because we know that all poisons may be qualified by many circumstances, so that the strongest may not have effect. A barrel of gun-powder may not take fire with a red-hot poker, under certain circumstances; that is, if by moisture you render it incapable of combustion. Thinks it scarcely probable, if contagious matter is in the bales of goods, unless the period of time is very great, that it could fail to excite the disease. The only way he can account for its not having taken place during the last 154 years in England, is, that it was never shipped from the Levant; but neither 154 years nor six or seven centuries can give the hope that it cannot exist in a British atmosphere, when we know that such a disease existed before. The plague of 1665 was no doubt the plague of the Levant, according to Dr. Mead.

John

John Green, Esq., Treasurer to the Levant Company.—Has been in Constantinople from the year 1774 to the end of 1780; six years. Has during that period seen a raging violent plague in May, June and July, by which upwards of 200,000 people died. It was he thinks in 1778, and was the greatest ever known till about five or six years ago, when there was one rather more violent. Thinks the plague is an epidemic, occasioned by a particular state of the atmosphere; and contagious so far, that if you come in contact with the person actually ill, there is the same ground for apprehension in that case, as in case of any fever. Does not think it can be communicated by the clothes or goods; by goods, certainly not. The clothes belonging to persons who die of the plague are sold; they never destroy them. Has never known the clothes to be the cause of the plague in other persons. It is a general remark, that the dealers in clothes do not take the plague. The natives do nothing with the clothes; the Europeans generally wash them, but there are few cases of plague among Europeans; the reason why he thinks they are not infectious is, that the plague frequently ceases suddenly; it ceases, and does not recur for two, three, four, or five years; and the clothes not being destroyed, but generally distributed and worn as well as the bedding—conceives that if they were contagious it would be impossible that we could be without the plague during that period—even the bedding is sold. There is a custom in Turkey, that if a stranger dies in the plague, the governor or pasha takes possession of his property, and the clothes are part of the property; and of course he orders them to be sold for his own benefit, and they dare not destroy them. Considers that the same person can have the plague more than once, and has known instances; but only from common report. It is the general belief of most people; but there is a particular symptom, that if a person has the plague with a particular species of buboes—they call it the Blessed—when they have had that, they are not liable to take it again so much; if they do, it is only slightly. Has heard that the Abbé who had the care of the Frank hospital at Constantinople had the plague ten or twelve times.

It has been generally conceived that the plague was put a stop to by extreme hot weather, or extreme cold weather, and thought so too till lately. Is of opinion it is not the heat, but the effect of the heat. It is the fall of the dew that stops it, because the plague prevails at Alexandria, in Egypt, occasionally, till the 24th of June; at that time the sun has such power, that it occasions strong exhalations; a strong fall of dew, almost like rain; and it is so much a matter in course, that the people, on the 24th of June, who had shut themselves up, came out without any apprehensions

hensions at that time. And about five years since we had very strong fogs here in London for about 14 days, so that we could not see across the street. At that time Mr. Green had a letter from Mr. Morier, consul general, dated, he thinks, in February; in which he stated, that the plague that had begun to be very prevalent had all on a sudden entirely ceased; and that he could not account for it, unless it had been occasioned by the extraordinary continuance of dense heavy fogs; but that it ceased. When Mr. G. received the letter, it occurred to him to inquire as to the state of Smyrna. At Smyrna it is expected to cease about the month of July, and generally does cease during the great heat. Inquired of a captain of a ship that had been many years in the trade, whether during the great heat at Smyrna, in the month of July, there was any appearance of dew. He stated, Certainly; and upon asking his reason for giving so direct and immediate an answer, he said he was certain of the fact, because during the hot weather the crew slept on the deck; but that in the month of July, when the sun became powerful, it occasioned such a heavy fall of dew, they were obliged to go below to sleep; it would have wet them through. Does not feel competent to decide why the dew has that effect. Always understood from the Armenians and other natives of Constantinople, that exposing clothes of infected persons in the night to the dew, would more effectually render them innoxious than putting them a week in the sun. During the six years he was at Constantinople the plague would cease, some months at a time; and it had not prevailed for two or three years when he first got there. Arrived in 1774, and to the best of his recollection the first instance of plague was the beginning of 1778. From 1774 to 1778 there was no plague in Constantinople; at Smyrna they have been without the plague for three or four years. After the Quarantine Act was passed in 1800, that is the first Quarantine Act, for the first Report made was in 1800, (the quarantine permitting ships to come from Turkey with clean bills of health was in 1800) Mr. G. predicted that we should not have any further foul bills of health, and for this reason. The bills of health are determined by the foreign consuls at Smyrna, upon the report of a number of Greek merchants who form a committee for the purpose. These merchants carried on principally the trade between Smyrna and Holland, that is, several were concerned; it was their interest to establish foul bills of health, in order to keep the trade to themselves; because English ships could not come to England without going first to Malta or Leghorn, or some other lazaretto in the Mediterranean, to perform quarantine of ninety days. In the mean time the Greeks loaded cotton wool and other goods, and all the articles
which

which constituted the chief object of the trade, in ships, which they sent to Holland: there they have no quarantine establishment. The practice in Holland is, to take a few of the goods out on the arrival of the ship, which they put into a lighter alongside the ship and cover up the hatchways; at the end of twenty-one days, the ship and lighter go up to the quays and discharge their cargoes; sometimes the cotton is trans-shipped in vessels bound to London without being landed. On their arrival in England they were liable to fourteen days quarantine in Standgate Creek, where they merely cut a little slit in one side of the bales of cotton; after the end of fourteen days, the cotton was sent to London in the same vessel or in lighters, and of course immediately sold and distributed among the manufacturers, without any other precautions than now stated. By these means the Greeks anticipated us, and we could not carry on the trade; but there is another reason now, to influence the bills of health. The committee who decided on the plague or no plague at Smyrna, during the time of the plague when it was known to prevail, collected from the Greek community a certain sum weekly. (Is not aware that they collected from other people.) That is, for the avowed purpose of rendering assistance to persons afflicted with the plague, but there is no account rendered of the distribution; consequently, so long as they can establish the plague to exist, they collect these contributions. On the other hand, when there has been no accident from the plague, and clean bills of health are issued; then the community resist as much as they can the first allegation of an accident from the plague, in order to save their contributions. The plague is said to originate sometimes in Smyrna. It prevails most in particular low narrow streets, where the houses are so close that you can shake hands across the way, and which are inhabited by the very lowest classes of people; a place into which no European would chuse to go, therefore we do not go to investigate it: it is a situation where fevers must necessarily be expected, from the confined air and want of ventilation, and the concourse of persons existing in such numbers there. Thinks the plague is not necessarily taken by contact; because during the plague in 1778, has seen the man who brought provisions to the house where Mr. G. lived a dozen times every day take off the bundle of clothes belonging to people who had died of the plague, Armenians. They are buried without coffins, but carried on biers; and when they are put into the grave, they bundle the bed and clothes into a sheet and bring them back again, bring them home. There was a winehouse opposite Mr. G.'s house; has seen the man who attended him take the bundles from the men's backs as they were returning from

from the funerals a dozen times a day, to go into the winehouse to get a glass of wine. Has seen him also very frequently assist people staggering in the street from debility, from illness to walk home to their houses during the height of the plague; remonstrated with him on the subject; when he said he did not trouble himself about the plague: he was very much in the habit of getting drunk, half drunk all day. Has seen him lie in the streets, and persons passing the dead over him, kicking him out of the way like a beast or a log. When the plague first broke out in 1778, the son of an Armenian merchant opposite Mr. G.'s house was taken ill; and a clerk in the counting-house of the Armenian was sent with this young man that was ill, to a place called Ortaquey, a village about four miles from Constantinople, on the borders of the Bosphorus, and there he remained for nine days. At the end of that time the young man died, and it proved to have been the plague; the clerk had attended him day and night, during the whole of his illness, and slept in the same room, on the same sofa probably, (for the sofas go all round the room,) and he was not at all affected by it, and did not take the plague. Considers there are often instances of the plague, without its spreading in the community. It has been a common observation, that if the plague exists at Constantinople and not at Smyrna, if persons infected with the plague go down from Constantinople to Smyrna, although they die there, the plague does not spread at Smyrna, and *vice versâ*. It is generally considered, that if it is carried from one place to another where there has not been the plague, it does not spread. There was an English ship, last year, or the year before, the *Smyrna*, Captain Farmer, carried down two Turkish passengers from Constantinople to Smyrna; one died of the plague, and the other was landed at the fort about seven or eight miles off Smyrna. The ship was, on her arrival at Smyrna, ordered by the Consul to perform 40 days quarantine, to be fumigated before they would permit her to take in any goods; but neither the captain nor any of the crew were affected by the disease; they did not take it. Believes there is no instance on record, of any English sailor dying of the plague on board the merchantmen in Turkey. Thinks their escape from it may probably arise, not only from the different habits, living freely and drinking wine; but also from the English sailors in general sleeping on board their ships, where there is a great difference in the atmosphere from what it is on shore, perhaps eight or ten degrees. Another circumstance is, all the European merchants in Turkey employ brokers, who do all their business, buying and selling for them; these persons go about freely during the plague, buying and selling goods, and collecting monies. Does not recollect any of these people taking the plague, except in two instances. Rather thinks
they

they did not use any precautions. There used to be a custom, as to the mode of receiving money, rather different from what it is now; there was a small half tub with water inside the railing in the court yard, into which the money was thrown. It was supposed any thing immersed in water would be purified; they used to throw in the meat, and every thing brought in the house, except the bread and flour; the bread is considered not to be capable of infection. The plague in Turkey almost always declines suddenly. Generally prevails most towards the winter at Smyrna, and Constantinople in summer.

A notion prevailed originally that the plague of Egypt was more dangerous than that which arises at Smyrna or Constantinople; and if a case happens, of a person infected coming from Egypt to Smyrna, and occasioning a foul bill of health, it has been generally believed that the plague would spread; but there is an instance, just occurred, which is directly in contradiction to that. About the month of November last (1818) or December, a foul bill of health was issued at Smyrna, in consequence of some persons arriving from Egypt infected with the plague; but instead of the plague spreading at Smyrna, the letters received to the 11th February state, that the vessels which had sailed a few days before had sailed with clean bills of health, and that the plague had not spread. Is convinced that the plague never has been brought from Turkey to this country, or to Holland, nor ever will be, by mere merchandize. Does not think that the plague was carried to Messina nor Marseilles by merchandize; because in both instances he had occasion to remark to the Quarantine Committee in 1800, that Dr. Russel, in his publication on the Plague, expressly stated, that the plague existed on board the ships at the time of their arrival in these places. The ship that was supposed to carry the plague to Messina came from the Morea, from a place where the plague had been very prevalent; some of the crew had died; she was only 36 hours coming from the Morea to Messina, and the captain himself was ill at the time, and he died within a day or two after he had communication with a person who smuggled on shore a box of jewelry. The ship that took the plague to Marseilles had loaded at a port on the coast of Syria, where the plague had not prevailed for two years. After she sailed from Syria a contrary wind forced her into another port on the coast of Syria where the plague had prevailed; and she took on board several Arabs, passengers, merchants, to take them to the island of Cyprus. Some of these persons were ill of the plague at the time, and died; after that the crew, some of them, took the plague, and they had put into more than one port, and had been driven from other places, and had the plague on board actually at the time of her arrival at Marseilles.

seilles. A passenger went on shore (for there was no quarantine establishment, at least it was not rigid); the passenger went on shore, and shortly after the plague appeared at Marseilles. Never heard the plague carried eastward from Smyrna by the caravans. Has heard it remarked that the plague did not extend beyond Turkey. Has never known any person who handled the goods in quarantine, infected in England. Attributes our never having seen the plague in England, for a great number of years, to the state of atmosphere. Does not think the goods which arrive in the quarantine establishment could produce plague. Should not apprehend the danger of the plague being brought in any way but by persons actually infected with it. Conceives the climate materially altered since the plague in London; not only the climate, but the circumstances of the country generally, and especially London itself; the improvements in London render it generally less liable to epidemic disease. Has many doubts as to the identity of the plague in 1665 with the plague in Turkey. The symptoms do not appear to be the same. Is by no means of opinion, however, that the performance of quarantine should be abolished. Thinks that many modifications may be established without any risk, but that it would be improper to abolish the regulations altogether; not only because it is necessary to have a proper examination of all vessels arriving, to see the state of health of the crew and passengers when they arrive; but because it is also absolutely necessary, that we should observe certain formalities of quarantine, on account of our connection with other countries where a more rigid quarantine is conceived necessary. If the quarantine establishments of this country were abolished, no matter why, it might occasion a prohibition of our vessels in other ports.

Being asked to state the process of ventilating goods,—answers: “The English lazarettos are old men-of-war, with houses built upon them like an ark; the sides of these houses are open like a brewhouse, with shutters, and the floors are all open gratings in fact, so that the ventilation is excessive on board these vessels in Standgate Creek, greater than it is possible to give on any building on shore. The ships also swing with the tide; that is, when the tide turns they change their sides to windward every six hours. Remembers Sir Gilbert Blaine and Dr. Johnson went down to Standgate Creek to examine the floating lazarettos, and they stated the ventilation was the greatest they had ever met with. Dr. Johnson told Sir Lucas Pepys, that the ventilation was greater than the north-west winds on the coast of America. In some species of goods the bales are ripped open on one side, sufficiently to let in the air, and for different periods of time, for common bills of health and foul bills; in one instance they undergo 15 days

quarantine, in the other 40. The internal part of every bale is not exposed; but certain articles are ordered to be emptied out of the packages: supposing a ship comes with a foul bill of health; supposing goat wool, the order is to empty it entirely, so that they ventilate the articles in bulk; but the bales that have been ripped open on one side, continue so for a certain number of days, when the side is sewed up again; and the other side of the bale is ripped open and exposed for a certain number of days, in like manner. The people who manage the expurgation of goods there, are ordered to push their arms in as far as they can; it is done for the express purpose of ascertaining whether there is infection. Corn is under a different direction. It is subject only to a nominal quarantine; they let it be taken out of the ships directly; they order a grating for the corn to pass through, in order that, if there is any loose rag, it should be stopped. The practice is this; the ship remains a certain number of days, and if all is well, they discharge the goods; and if there are bags or mats, they are taken out. Thinks that if goods which arrive are capable of communicating infection, it is not possible the expurgators could escape it; and that no infection could have existed for the last 200 years in England. Is of opinion that the typhus fever is the plague in a less degree. Looks upon it, it is the same disease, only that in Turkey, from the different habits of the people, there it acquires more violence: they live upon fruits, cucumbers, melons; eat very little animal food, and drink chiefly water, which in summer time is stagnant. Constantinople is supplied by water from a lake at a village called Belgrade, about 16 miles from Constantinople, by means of aqueducts, tubes or pipes underground, that were constructed by the Greeks and Romans. In the summer time that lake is very much dried up and exhausted; and the water becomes so bad in the public fountains, that very often the Turks come to beg rain water from the tanks of the Christian merchants; rain water that they had saved. Recollects about 1800 or 1801, there were three ships' cargoes sunk at the Nore. The circumstances were these: the ships had loaded at Mogadore, where the plague prevailed to a violent degree, so much as almost to have depopulated the place; and the cargoes consisted chiefly of goat skins, which had been collected during the time of the plague; they were turned with the hair inside, and they were packed in bales. Remembers stating at the time, that he conceived, if danger of the plague could exist at all in merchandize, it would certainly be in these goat skins, as they were incapable of being opened and aired unless every skin was turned out again, which would have been a very dangerous operation; and therefore, as they could not be expurgated by the existing regulations of quarantine, he submitted,

mitted, that if danger of infection could exist at all, it might not be correct to subject any individual to the process of turning those skins. That was his opinion at the time; and after having discussed the subject, and taken the opinion of the persons supposed to be most competent to judge, the ships and cargoes were ordered to be sent down to the Nore, and sunk in deep water. Mr. Pitt seemed to be very reluctant to have these ships destroyed. They were from 120 to 150 tons. Government paid the value of them. Thinks the value amounted to more than 20,000*l.* but can only speak from conjecture.

Dr. John MacLeod.—Has served much in tropical climates, and in the Mediterranean, but doubts much whether he has seen any case of the plague. Conceives that any fever may become contagious when people are crowded up together; under bad management, without ventilation and cleanliness, a fever will become highly contagious. At the same time believes we have much less to apprehend from contagion than is generally believed. Had lately an opportunity of witnessing a very bad fever at Batavia, when under circumstances extremely favourable for the operation of contagion; although the ship's crew were very crowded, having been lately wrecked, and at the time huddled together in a transport, yet, by using proper means, such as free ventilation and doing their best to prevent the accumulation of morbid effluvia, no case appeared to arise from infection: those men who were ill, having evidently become so by sleeping in Batavia, and getting drunk there. Thinks the plague should be considered under the general description of fever. People are said sometimes to die before the usual febrile action takes place, but that does not take away the character of fever which it possesses generally. Does not consider that the quarantine establishments in this country have prevented the infection of the plague; because the plague has never made its appearance through shipping. Quarantine could not certainly have prevented men arriving with the plague on our coasts; and the expurgators, or men employed in opening the goods, must have been attacked by the disease at one period or other, had it been possible to import it in this way. Does not know that any thing more is necessary than to inquire of every ship that arrives, whether they are all well on board; and if any men are ill of fever, to treat them as you would other people. Would put them in an hospital, tent or barn, and treat them as rational beings, and not like mad dogs, by cutting them off from society and assistance, and exciting fear and alarm. The depressing passions are much to be avoided. Can see no reason why seamen should be used in the common rigorous way, when they happen to arrive with fever, more than persons in Birmingham and Manchester, where there are typhus fevers prevailing

every year. In Dr. M.'s mind it is fully established, that we have nothing to fear from the importation of the plague from the Levant to London; and he grounds this opinion upon the simple fact of long and great intercourse, without the disease having once made its appearance by a ship. We have tried the experiment sufficiently long to be satisfied that we have nothing to fear from the importation of goods. Considers the cause of the plague to be the same as that of any other malignant fever, foul effluvia, dirtiness, want of ventilation, and poor living; you may generate in this way, a fever like our gaol fever.

John Green, Esq. again called and examined.—Has known of instances of persons having slept with others, and not having taken the plague. Mr. Slaars, a Dutch merchant at Smyrna, had two daughters who slept together; the one was taken ill, and the sister continued to sleep with her; at last she died, and upon examination it appeared that she had had the plague; the sister did not take it, nor did any of the family.

Mr. Perkins, an English merchant at Smyrna, had also two daughters who slept together; one of them was taken ill; it appeared that it was with the plague; she got well of it, and the sister did not take it, nor did any of the family. Previous to 1800, English ships were not permitted to come direct to England from Smyrna without a clean bill of health. When they had not a clean bill of health, they went to Malta or Leghorn to perform quarantine, and afterwards shipped the same cargoes on board them to England. In the year 1800 an Act was passed, permitting English ships to come directly to this country without a clean bill of health. There was a general revision of the quarantine laws in 1800; a committee was appointed for the purpose of making a report upon certain questions specifically put to them by the Privy Council; the Privy Council afterwards formed regulations respecting quarantine generally, including ships coming without clean bills of health.

Being asked, If under the form of regulations which prevailed before 1800, it is not probable that the plague was seldom if ever shipped for England from the Levant?—answers: “I cannot speak as to the shipment of the plague; I can speak specifically, that the same species of goods had been for a century brought to this country from Smyrna, during the plague, by ships bound to Holland, and from thence the goods were brought here; they have no quarantine establishments in Holland, consequently it was tantamount to their having come direct.”

Sir Arthur Brooke Faulkner.—The only opportunity he has had of seeing the plague, was in the Island of Malta in the year 1813. Was physician to the forces, and the only staff physician employed during the greater part of that service. Believes the plague

plague is generated or produced by a contagion *sui generis*, quite peculiar and specific, and that it is communicated only by contact or close association with the person or thing infected. It was communicated in the first instance to the Island of Malta, in the direct line of contact. It could be traced to have been propagated in the direct line of contact, in the city of Valetta, and from the city into most of the cassals or villages, where any history could be obtained of its introduction. The first case of the communication of the plague was, in his opinion, from a vessel, the San Nicola, in the harbour, to the family of a person of the name of Salvator Borg. The vessel was lying in the harbour contiguous to the city of Valetta; the harbour is called Marsamuchetts. The daughter of Salvator Borg died with well marked symptoms of the disease on the 19th April. Two other persons of the same family died on the 2d May, all with well marked symptoms of the disease. From the family of Borg it made its way in a direct line into the family of one Maria Agius, a school-mistress, who, together with others she immediately communicated with, were attacked by the plague, and all of whom (with some of her scholars) were seized or perished with well-marked symptoms of the disease. The *foci* of contagion became so rapidly multiplied, that it appeared impossible to carry the investigation in a direct line any further. The means of its communication to the small contiguous island of Gozo, at a late period of the calamity, can be distinctly made out. A man belonging to an infected family in one of the cassals, made his escape with a box of clothes into a neighbouring cottage; it was speedily found out that he had escaped, and he was accordingly apprehended and sent to the lazaretto. On his enlargement from the lazaretto, he returned to his cottage, where he took this box of clothes that had never been suspected to be there, but had been concealed; and he hired a boat and carried this box of clothes to the island of Gozo. The first family infected on the island was the family at whose house he arrived, and to which place he carried the box of clothes. It was a marriage present; and a priest acquainted in the family, was one of the first victims; he died with well-marked symptoms of the plague. The plague was not at all particularly prevalent where the marsh fevers were most generally produced. Never could trace any series of symptoms that could lead him in the slightest degree to suspect any identity between them; but has known the plague to personate in certain symptoms almost every possible form of fever, and has known it to be entirely free from every kind of fever; there is no certain type to which it can be affixed. Means to say, that fever is not an essential attribute of the plague; it is frequently mortal where there is no fever. It is an extraordinarily anomalous disease, which has almost defied definition. The disease did not

extend to Sicily, in consequence of the prompt precautions that were adopted: and, had there been the same at Malta, is persuaded that the disease would have been resisted *in limine*. The plague has been known to be received in the lazarettos; has heard many instances related by the Maltese, and produces evidence of one; the title page of a book, which represents a monument raised to the memory of a grand master, for having arrested the disease in the year 1743.

Being asked, If he knows any thing of the introduction of the former plague that ravaged Malta,—begs to refer to a paper he published on the disease, during his engagement, on the plague at Malta, which was communicated to the Edinburgh Medical and Surgical Journal, and published 1st April 1814. Read the following passage to the Committee: “It is somewhat remarkable, that the history of the introduction of the plague, when it made such great ravages on the last occasion on the island, about a century ago, was nearly similar to what is circulated of the present; being attributed to some linen brought from a Levant vessel, by a Maltese shopkeeper; which, after producing the disease in all those who first came in contact with it, ultimately disseminated the malady throughout the whole population.”

During the late plague at Malta in 1813, the disease was arrested from the moment that an adequate and a regularly organized police was established, and the inhabitants shut up in their houses, and other strict measures of quarantine enforced, (which was the case at a very late period) in the month of August. From the 16th August it went on decreasing on the average, but not regularly, till it disappeared. Feels satisfied that the decline of the complaint was very much owing to the prompt measures of police; and his reason is, that the thermometer rose inconsiderably in point of fact, while the disease was decreasing fast. During his residence at Malta it did not appear to him that the temperature of the air had any thing to do with the plague. Is of opinion that a very high or a very low temperature would check it. Believes that materially below 60 or probably at 60 degrees of heat it cannot subsist; but it is bare conjecture. Thinks the plague principally communicable by the touch. States the following circumstances as to the degrees of precaution used in each of the military barracks. The Sicilian regiment, though situated in a very infected part of the island, a place called Florian, escaped by the promptness and vigilance of Col. Rivarolla. De Rolle's regiment, which was in the healthiest spot, was invaded by the disease, and evidently in consequence of their barrier admitting a contact with persons on the outside. It was a barrier at which you could shake hands with any person on the outside. In the 14th regiment, which was near the most unhealthy part of the town, there was but one person suspected, and his disease was
immediately

immediately arrested; the public prison and public general hospital escaped. The convents in Valetta escaped, with the exception, it is believed, of one; and the introduction of the disease to that one was accounted for. The prison and these public institutions escaped, Sir A. B. F. conceives, very much by the voluntary attention paid by their inhabitants to a strict system of quarantine. Thinks the plague can be propagated from goods as well as persons. Gives the following as his reasons for thinking why the plague has not been introduced from goods in the quarantine establishments. In the first place, quarantine restrictions since 1709 have been a great deal more rigid; indeed they did not exist in England at all, previous to that period. Conceives that the intensity of the contagion may have been greatly blunted by the length of the voyage, and the length of time that passes after the shipment of goods. Besides, we know, that other countries have a good system of quarantine, which is in favour of the plague not being imported here. Is inclined to consider the plague of 1665 to have been the true Levant plague. Can account for the expurgators never having taken the plague, only by collateral considerations. 1st. That we have observed, in other countries, the disease has not taken place for a long series of years, not for 130 years in Malta. 2dly. We do not know what the circumstances are that constitute aptitude in the receiver, sufficiently, to know why the plague has not been received into the lazarettos since 1665. But 3dly, it does not follow, because it has not been received into the lazarettos since 1665, that it may not by some fortuitous concurrence of circumstances occur again here. It has been stated, that the ancients were not acquainted with contagion, but adduces instances from the medical writers and the poets, to the contrary.

Refers to the following, viz.

Συνδιατρέβειν τοῖς λοιμώττουσιν ἐπίσφαλες ἀπολαῦται γὰρ κίνδυνος, ὥσπερ ψώρας τινος ἢ ὀφθαλμίας.—Galen, lib. 1. cap. 2. *de differant. Februm*.

Διὰ τί ἀπὸ μὲν νόσων ἐνίων νοσοῦσιν οἱ πλησιάζοντες, ἀπὸ δὲ ὑγείας οὐδεὶς ὑγιάζεται;—Aristotle, *Probl. lect. vii. 1.*

Δέος δὲ ξυμβιῶν τε, καὶ ξυνδιαίτασθαι, οὐ μείον ἢ λοιμῶν ἀναπνοῆς γὰρ ἐς μετὰδοσιν, ῥηϊδίῃ βαφῇ.—Aretæus *de Elephantias.*

Infecti quasi valetudine et contactû.—*Annal. Tacit. l. 6. & 7.*

Postea curatio ipsa et contactus ægrorum, vulgabat morbos.—*lib. 25 & 26.*

Contagion is clearly expressed in the last eight lines of the third Georgie of Virgil; likewise in the first Bucolic, verse 52; where these words occur:

Nec mala vicini pecoris contagia lædent.

There are numerous other authorities.

[To be continued.]

LXXII. *Memoir of the late JAMES WATT, Esq. F.R.S.**

JAMES WATT, the great improver of the steam-engine, was born at Greenock in 1736. His grandfather and uncle were both distinguished as mathematicians and land surveyors: and the latter was the author of a Survey of the river Clyde. His father, James Watt, was a merchant and magistrate of Greenock, and a zealous promoter of the improvements of that town.

Of the early years of the subject of this memoir, we shall only state that he was educated in the public schools of his native town, and that his constitution, which was of the most delicate kind, led him, even in his boyish days, into those habits of studious retirement which accompanied him through life. His partiality for the scientific arts soon developed itself, and at the age of eighteen he went to London, and placed himself under the tuition of an eminent mathematical instrument-maker; but ill-health occasioned his return to Greenock in about a year, and this appears to be the only instruction he ever received. All the rest was self-acquired: but so early had his talents developed themselves, that in 1757, when he was in his 21st year, he was appointed mathematical instrument-maker to the university of Glasgow, with apartments in the college. From these he removed to the town of Glasgow in 1764, upon his marriage with his cousin, Miss Miller; and in that and the subsequent year he invented his improvements on the steam-engine. He soon after formed a connexion with Dr. Roebuck, of Kenniel, near Burrowstoneness, to carry the invention into effect; but circumstances delayed his taking out a patent until the year 1769. From the time of this invention, until the year 1774, he followed the profession of a civil-engineer, at Glasgow, and made many surveys of canals and harbours; several of the former of which have since been carried into execution. The fortunate circumstance of his undertaking the repairs of a model of a steam-engine, in itself of so little importance, led to one of the greatest revolutions in mechanics that has taken place by any one invention since the records of history.

The invention of the plough, of the saw, and the application of the power of horses or oxen to turn mills, took place in such early ages, that those persons to whom mankind are indebted for them, are not known either by name or by any authentic records; but we know that they were considered as demi-gods, and honoured as such. The records of those inventors are fabulous in their details, but they are true as to the reality of the discoveries; nor is it wonderful that their origin should be unknown, when even

* From the New Monthly Magazine and Literary Panorama.

the invention of the first steam-engine is not well ascertained, although certainly not two centuries ago.

Men at first were obliged to supply all their wants by their own labour. The next advance towards our present state was the employment of horses and oxen. Minerva was worshipped under the name of *Boormia*, for having first taught the yoking of oxen to a plough, and horses to turn mills for grinding of corn.—Diodor. book iii. chap. 64.

The elements were next called in to aid men in their labours, and the powers of air and water were employed for those purposes that had in earlier times employed the most delicate and feeble portion of the human race*.—But falls of water are only to be found in certain situations, and the “wind bloweth as it listeth.”—It was reserved for the inventor of the steam-engine to create a power that could be at the command of man, without regard to time or place, such as we see it in our day.

Though the inventor of the steam-engine is not known, about the middle of the seventeenth century, the Marquis of Worcester mentions and describes a machine in his book, intituled “The Marquis of Worcester’s Century of Inventions,” which was published in 1663. The noble marquis does not claim the invention, and there were numbers of inventions, of which he gives an account, that were not his own. Though the marquis does not claim the discovery of the principle, yet he distinctly says that he had invented methods of applying them to the raising of water. His description of the mode he employed, is not sufficiently distinct, to be well understood. He employed two vessels, that filled with water, and acted alternately, the water being ejected with a force that made it spout to the height of forty feet.

It does not appear that the marquis, the greatest inventor of his age, did actually employ steam as a power, for any useful purpose; but in the beginning of the following century, Captain Savary constructed a machine, of which steam was the acting power, for the purpose of raising water. The captain obtained a patent for his invention, and employed his machines for draining the water that accumulated in the mines of tin and copper in Cornwall.

The success of Savary was not complete; but the attention of ingenious men was called forth, and with the aid of Mr. Newcomen and Mr. Crawley, the former an ironmonger, and the latter a glazier, at Dartmouth, in Devonshire, an engine was con-

* Antipater, of Thessalonica, addresses the female sex thus—“Women! you who have hitherto been employed to grind the corn, for the future let your arms rest. It is no longer for you that the birds announce by their songs the dawn of the morning. Ceres has ordered the river nymphs to move the heavy millstones, and to perform your labour,”

structed, that went by the name of Newcomen's engine, which continued to be in use for the draining of mines, from 1705, till Mr. Watt's great improvement, of which we are going to speak.

Savary associated himself with Crawley; and the alteration introduced by the latter, and by Newcomen, who probably was the working engineer, was, that Savary, like the Marquis of Worcester, raised the water by the pressure of steam. Whereas, in Newcomen's engine, which they constructed and sold, the steam was only employed as the means of procuring a vacuum in a cylinder, in which was a piston, which was pressed down by the weight of the atmospheric air. The piston was attached to a lever or beam, at the other end of which was a weight, that raised the piston as soon as there was a fresh quantity of steam let into the cylinder, under the piston. When the piston got to the top, a jet of cold water was introduced, that condensed the steam, and again producing a vacuum, the piston descended by the pressure of the atmosphere.

Much time was employed in contriving methods to open the cocks necessary; and it was not till 1717 that the ingenious Mr. Boughton produced a machine, where all the operations of turning the cocks were performed by the engine itself.

Had there been any other power capable of draining deep mines, so expensive and complicated was this engine, that probably it would never have been brought into use; but necessity obliged the miners to persevere in employing it, and constant endeavours were made to reduce the expenditure on consumption of fuel, though without any further progress than constructing the boilers and fire-places under them, in a manner to lose less of the heat.

Mr. Watt was, by natural genius, an inventor, and, by his education and research, led to seek for œconomy and perfection in the machine, not by trifling improvements, but by a great alteration in its principle.

The original inventor of the steam-engine undoubtedly laid the foundation for all the wonderful effects that are now produced by that mighty machine; but while it was merely employed for the drawing of water from mines, it was of but little importance, compared to what it is at the present day: and that change, from a small degree of importance, to that of the very first-rate degree, was exclusively almost the work of Mr. Watt.

We must distinguish the steam-engine from all other machines, whether for spinning thread, or rolling iron, &c. as they are all, without exception, however useful and ingenious, the means of applying power; but the steam-engine creates the power that it applies, and in that is different from all other human inventions*.

* Although seventy or a hundred horses may have a power equal to a large steam-engine, how could such a number be employed in giving motion to one machine? The impossibility of producing equal or simultaneous effort to so great a number is obvious.

The steam-engine had been for a century, or nearly about that length of time, employed for drawing water; and such was the expense attending it, and the difficulty of keeping it in repair, that it was never employed where wind, water, or animal force, could answer the purpose.

In this situation was that wonderful machine, when the model was brought to Mr. Watt, of Glasgow, to be repaired, and put in working order, for the instruction of students at the college there.

Mr. Watt soon observed that there was a great waste of heat, and consequently consumption of fuel, occasioned by condensing the steam in the cylinder in which the piston works. That cylinder being of cast-iron, was cooled by the same jet of water that condensed the steam, so that when the fresh steam was admitted, a great quantity was consumed in again heating the cylinder; for steam cannot exist in its rarified state, or, more properly speaking, cannot exist at all in a vessel that is not nearly as hot as itself.

Mr. Watt calculated that about two-thirds of the steam that was introduced, was consumed or condensed by the coldness of the vessel; and his first expedient to prevent that waste of heat, was to have a wooden cylinder, which, being a less powerful conductor of heat or cold, would not be liable to the same disadvantage in an equal degree. He was perfectly right; but he soon found that wood was not a fit material in other respects. When heated, and subject to continual friction, it was too rough; and there were other inconveniences which made him abandon that plan, but without, for a moment, abandoning his endeavour to improve the machine.

It was then that his great genius exerted and developed itself, and he was inspired with the happy idea of permitting the steam to pass into a separate vessel, there to be condensed; so that the jet of cold water never being introduced into the cylinder where the steam was to be admitted for the next stroke of the engine, that cylinder was never cooled, and consequently the fresh steam was not three-fourths consumed in bringing it back to its proper heat.

When this was done, the grand improvement was made; but the difficulties of the inventor were only beginning. He knew well the value of his discovery, but to make others sensible of its value and obtain the means of bringing it to perfection was the great point. Mr. Watt was not only a modest, but he was what is termed a bashful man, and but few persons were capable of appreciating his merit*.

* When his great success—when the works he had performed convinced the world of his uncommon merit and genius, this very bashfulness was in his favour, and made him more highly esteemed; but it was a sad impediment in his way at first. Men with great pretensions and assurance obtain more credit at first than they deserve, but modest or bashful men less.

A gentleman

A gentleman of some property and considerable knowledge, Dr. Roebuck, who was capable of appreciating the merit both of the inventor and of the invention, at last united himself with Mr. Watt in the enterprise of bringing the matter to perfection; but his means were unequal to the purpose; and after expending all he could afford, the affair was on the point of being abandoned, when Mr. Boulton, the great Birmingham manufacturer, heard of the invention.

Few men were more capable than Matthew Boulton of appreciating the value of Mr. Watt's discovery, and none more disposed to engage in a liberal manner in the enterprise.

To a generous and an ardent mind, Mr. Boulton added an uncommon spirit for undertaking what was great and difficult. Perhaps if Mr. Watt had searched all Europe he could not have found another person so fitted in every way to assist in bringing the invention to bear. Mr. Boulton had money at command; he was a man of address and influence, and advantageously known to the world: in short, he was just the person who was wanted; and after reimbursing Dr. Roebuck for his expenditure and loss, he became partner with Mr. Watt, who removed to Birmingham.

The difficulties to be overcome were, however, immense. The expensive engines then in use could not be altered. It was necessary to erect others entirely new, if the proprietors of mines would turn the new invention to their profit.

Messrs. Boulton and Watt, for such was the firm, began by erecting an engine at Soho, near Birmingham, and exhibiting it to all those concerned in mining. They even went so far as to erect, at their own expense, engines on several mines, to be paid provided they answered the expectations they entertained.

A set of experiments was made under the eyes of several persons of well known honour and skill, to ascertain the saving made on trial by the employment of the improved engine; and it was found to be fully equal to what Mr. Watt had calculated.

The difficulties then began to diminish, and the proprietors of mines in Cornwall, where coals are very dear, were induced to make erections of the newly-constructed engines in place of the old, engaging to pay one-third of the advantage or saving in coals, for the liberty of working Mr. Watt's engine, for which he had obtained a patent, prolonged by an act of parliament.

The mine of Chace-Water was one of the first which had three of the largest-sized engines erected, for each of which the proprietors engaged to pay 800*l.* a-year, being one-third of the saving calculated by the price of coals.

Mr. Watt was not only an inventor in mechanism, but his spirit of order as a man of business was uncommonly great. The manner of settling the sums to be paid was highly ingenious.

From

From the depth of the pit, the size of the pumps, and number of strokes of the engine, they knew the quantity of coals that was necessary either for the old engine or the new. They therefore had only to estimate the quantity of coals saved in making a certain number of strokes, and according to the price they knew what was the value saved. A counter, to tell the number of strokes, inclosed in an iron box, was fixed on the beam of each engine; there were two different sashes to the box, and two keys kept by the proprietors and two by Boulton and Watt, who had a traveller that went round to the different engines from time to time, and the counters being examined, the money was paid according to the number of strokes.

Men with less perseverance or less genius for overcoming difficulties, would have failed in this enterprise; but still nothing could equal their patience and continued attention to the business.

In relating the important facts concerning so important an invention, we must not, however, omit to state some mistakes that were committed, for which it is not easy now to account.

In 1779 the elder brother Perrier came over to Birmingham from Paris to get an engine for the supply of that capital with water, and the whole was sent over with the permission of government. The engine at Chaillot was that then sent, after which the Perriers made several other engines, and, to do them justice, they executed them very well: but we beg the reader to attend most particularly to the fact, that the first engine erected at Paris was made at Birmingham, and sent over for M. de Prony, a French engineer, and a man of merit, who wrote a History of the Improved Steam-engine, which invention he gives to the Perriers, never once mentioning the name of Mr. Watt. The work is in two volumes in quarto.—M. de Prony is a man of merit, and well known to the literary and scientific world, and it is an injustice to Mr. Watt and his country that ought to be recorded.

Since we are led to this subject we must be permitted to add, that we were acquainted with the Perriers—that the elder brother was not a mechanic at all, and the other merely a good practical workman.

It is not very easy to conceive how M. de Prony could have committed so flagrant an injustice, because he must have known better, and therefore ought to take shame to himself, as every man ought who imposes falsehoods on the world for truth, which is in itself a great disgrace, but doubly so when by that falsehood he deprives a man of merit of the credit due to him, and gives it to another, who has not only no claim, but who was not capable of any invention of the sort.

Mr. Watt was too inoffensive a man to attack de Prony; and when the injustice done was mentioned to him in 1810 or 1811,
when

when he was in London, he said it was true, but that he had seen De Prony, who had made some sort of an apology, or entered into an explanation. Mr. Watt did not appear to wish to enter on the subject; but, with great deference to his opinion on that point, we think the injury neither admitted of explanation nor apology of any sort. It was a wilful and gross misrepresentation, and it can never pass for any thing else. What adds to the injustice is, that M. de Prony could not write a history of the invention for which he had not materials, though he has contrived to fill two quarto volumes with his account of that machine.

An injury done at the expense of truth deserves to be noticed in a way rather different from that in which Mr. Watt noticed the act committed by M. de Prony. We, however, go further, and say, that it is in some degree a national affair, and strongly suspect that it was in order to rob England, and not to rob Mr. Watt of the invention that the merit was given to the Perriers.

It is to be hoped, that as both Mr. Boulton and Mr. Watt left sons, they will cause this matter to be set to rights.

The steam-engine, notwithstanding the great improvements made, was only hitherto (in 1780) employed to raise water, and when intended to give motion to mill works, the water raised was made to turn an overshot wheel in the ordinary way; but this was attended with a great loss of power.

We now come to the second great improvement made on the steam-engine by Mr. Watt—that improvement which led directly to the revolution that has taken place in the mechanical world.

To convert a reciprocating motion into a rotative one, as is done with the common spinning-wheel, and in turning lathes moved by the foot, merely by means of a crank, might appear to be a very simple matter. In short, it might appear to be an affair already settled; yet this application of power on the great scale gave rise to some very curious circumstances.

During the summer of 1780, Mr. Watt set seriously about applying the engine to the turning mill work, which he intended to do by means of a crank. This is a mistake. The most complicated mode did not occur first. The application of the crank in the manner of the common turning lathe was Mr. Watt's first idea. But, to use his own words, as the rotative motion is produced in that machine by the impulse given to the crank in the descent of the foot only, and behoves to be continued in its ascent by the momentum of the wheel, which acts as a fly, he was unwilling to load his engine with a fly heavy enough to continue the motion during the ascent of the piston, and therefore proposed employing two engines, acting upon two cranks fixed on the same axis at an angle of 120° to each other, and a weight placed upon the circumference of the fly at the same angle to each of
the

the cranks ; by which means the motion might be rendered nearly equal, and a very light fly only would be requisite. This was only a project, and Mr. Watt's subsequent invention of the sun and planet wheels, and his application of the double engine rendered unnecessary the counter-weight, and produced a regular motion with a light fly.

While this model was making, a rumour went abroad that a Mr. Rickards was erecting in Birmingham a corn-mill, to be moved by steam, the engine being of the old construction, and not of the sort improved by Mr. Watt.

Owing to a sort of irregularity that very often attends men of genius, the model of the rotative motion with the cranks, was left for several months in an unfinished state, though the intention of completing it was not given up. In the beginning of the year following, Mr. Wilkinson, the great iron-founder, who cast all the cylinders and large pieces of the engines for Messrs. Boulton and Watt, called on Mr. Watt and told him he had contrived to get admission into the corn-mill, that Mr. Rickards had by that time completed.—He described how it was constructed, and Mr. Watt at once found that Rickards had got hold of his plan with the crank. The axis that made two revolutions for each stroke of the engine, and the fly with the heavy side—there could be no doubt that it was a copy, and that the plan had by some means or another been stolen.

Mr. Watt, immediately on the departure of Mr. Wilkinson, told his draughtsman what he had learned, and there remained no doubt of the plan having by some means been obtained. The draughtsman who had directed the making of the model, was anxious for his own sake to get at a knowledge of the manner in which the invention had been stolen ; he therefore in the first place got, by bribing one of the workmen, into the mill, and saw it was an exact copy of Mr. Watt's model, so that there was no doubt but that the plan was stolen. As only one workman was employed on the model, and that in a shop where there were no others, there could remain no doubt that that workman was the person who had communicated the plan.

After a great deal of trouble, the man was got to confess that he did give the plan to one of Rickards's workmen.

A patent, however, was obtained for the producing a rotative motion by means of a crank, in the name of Rickards ; so that Mr. Watt was prevented from employing the means he had himself invented, and he was under the necessity of finding out another method of producing the same effect. In this he succeeded in a most ingenious way, but by a means that it is difficult to describe ; it was called the sun and planet motion, and answered perfectly well.

We are minute in describing this, to show the difficulties inventors have to encounter; but most of all to prove, that in invention the most complicated and difficult method is that which generally occurs first.

The example of the common spinning-wheel, with the crank and heavy broad rim, which acts as a fly, was all that was necessary to be copied, and the business would have been done. To that did it come at last, after great trouble and expense, and indeed every improvement in the steam-engine was attended with immense trouble.

It was to this application of the rotative motion, that the world owes the general use to which the power of steam has been applied, and by which an almost total change has been produced in the mechanical world.

Many operations are now performed by the power of steam, that could only before be done by human strength, and still more by what horses were employed to perform. But that is not all; for where great power was wanted, neither men nor horses could be so applied as to do the business, and in most cases wind and water were out of the question.

Power can now be created where and when one pleases, and in the quantity that is required; and it has been calculated that the labour of three millions of men is performed by means of steam; but even that, though a most incalculable advantage, is not all, for operations are performed by steam that could not be performed by any other means with which we are acquainted. Deep mines could not be drained by animal force, and neither wind nor water could be applied, so that they must have been already abandoned in many cases, and by degrees in all.

It is probably, we might almost say certainly, to the improvements in the steam-engine, that this country owes its ability to have supported the struggle she had to maintain against nearly the whole of Europe and the United States of America at the same time*.

* An anecdote has been recorded of Mr. Boulton, which deserves record. —He was a man who mixed with the world, and went occasionally to court, where he was always particularly noticed by His Majesty.

Soon after he was connected with Mr. Watt, he appeared at St. James's on a levee day. "Well, Mr. Boulton," said the king, "I am glad to see you. What new project have you got now? I know you are always at something new!!" "I am," said Mr. Boulton, "manufacturing a new article that kings are very fond of." "Aye, aye, Mr. Boulton, what's that?"—"It is power, an please Your Majesty."—"Power! Mr. Boulton, we like *power*, that's true; but what do you mean?"—"Why, sir, I mean the power of steam to move machines." His Majesty was pleased, and laughing said, "Very good, very good; go on, go on." His Majesty little thought that Mr. Boulton was manufacturing a *power* that would enable him to resist nearly all the world in arms, but which turned out literally to be the case, though neither His Majesty nor Mr. Boulton saw the happy end of the contest.

The application of steam to the turning machinery, by converting the reciprocal motion into a rotative one, was but imperfect while the rod of the piston was connected with the beam or lever of the engine by a chain, for though the chain can draw, it cannot propel or push.

By one of the most ingenious of all inventions, which, however, cannot be described without the assistance of a drawing, Mr. Watt contrived to make the pull and push of the engine act always in a perpendicular direction, though the end of the beam moved in a circle. This invention was like all the others exclusively Mr. Watt's, though unlike some of the others, it was not pirated or stolen.

At the same time that this gave to the engine an accuracy and certainty of motion, it rendered the machine much less expensive. The two circular heads to the beam or lever became unnecessary, so that a simple straight lever, either of wood or of cast-iron, answered the purpose.

In Newcomen's engine, of a large size, the beam was five or six feet in depth, and three or four in thickness, built of 12 or 16 strong pieces of timber, costing seven or eight hundred pounds sterling.

The improved engines not only do not consume above one-third of the quantity of coal, but the boiler, the fire-place, and the building are all much smaller in size, and consequently much less expensive.

We have been particular in giving in detail the difficulties Mr. Watt had to encounter, and the wonderful genius and perseverance by which they were overcome.

Any person who sees now with what facility the engines are managed, and the perfection with which they are made, must feel a *difficulty* of conceiving the *difficulties* that good workmen and men of genius found in managing the machine, and making it perform well for some years after its first invention, but that only proves the truth of an old proverb, that *Practice makes perfect*.

Never was there a more happy or fit union of two persons acting in conjunction than was witnessed in Messrs. Boulton and Watt. Though of very different manners and habits, no two men ever more cordially agreed. Mr. Watt was studious and reserved, keeping aloof from the world, while Mr. Boulton was active, mixing with people of all ranks with great freedom, and without ceremony. As both were highly honourable men, and very attentive to business, they did full justice to every thing they undertook, and to every person with whom they transacted business.

They were liberal to those whom they employed: and Soho,
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where their manufactory was carried on, became a seminary for engineers and mechanics.

Amongst the persons who rose to eminence by their means is Mr. Rennie, the engineer, who has so highly distinguished himself by the great works he has planned and executed.

We must be permitted to digress a little on this occasion, as the subject is important, and the opportunity one that is very appropriate.

It has often been regretted that we have not in this country a regular school for civil engineers, as they have long had in France; but if we may be permitted to judge of the tree by its fruit, this is far from being a disadvantage.

The School for Roads and Bridges (*l'Ecole des Ponts et Chaussées*) has not produced any such men as Brindley, or Watt, or Rennie, or Maudsley, or Brunel, all men who have been irregularly bred.

The bridges constructed in Paris in latter times are far inferior to those constructed in London. The iron bridge, for example, opposite the old Louvre, is ugly and ill built. Neither the design nor the execution are tolerable, and it is as far inferior to the Southwark bridge as a cottage is to a palace, though the execution was far less difficult, and there was a fine opportunity of showing how a bridge of one single arch might have been thrown over that narrow river, where the abutments at both ends were of equal height of solid stone, and high above the river.

As to the high-ways in France, they are not improved in the least degree since Louis XV. ordered them to be made more than twenty years ago.

We can only account for this by engineers following a routine when bred in schools, and by youths being educated for a profession that requires genius, before it is known whether or not they are possessed of the genius requisite.

When there are no schools there is no routine to follow; and those only are employed as engineers who distinguish themselves by their ability.

Monsieur Rich de Prony, who has acted with such injustice to Mr. Watt, in giving his invention to a man who never invented any thing, is at the head of that department; yet with all *appurtenances and means to boot*, he has produced nothing. The bridge of Nully was built by Perronesy, and none of the bridges built since over the Seine are any way equal to it.

As to architecture, if we are to judge from the recent structures in Paris, it is on the decline. None of the new works are equal to the old Louvre, or the Porte St. Martin and St. Dennis. The triumphal arch of the Place de Carousel is a bad copy, from
a work

a work of the same sort at Rome, covered profusely with admirable sculpture in miniature ; it produces no striking effect, but looks like a model badly planned, and badly placed, but carefully executed.

None of the new fountains are equal to the ancient one of the Innocents ; and if we were to follow the comparison throughout, we should find a similar falling off.

England is not a country for architecture ; we want the fine material ; yet how much inferior is the church of St. Genevieve to St. Paul's !

As to canals, the French seem to retrograde, and forget what they formerly knew. The canal of Languedoc is a great work, but all the canals of a late period are badly contrived, and as badly executed.

In 1779, while Mr. Watt's grand invention of the steam engine was gradually improving, he invented the machine for copying letters, by means of a thin moist paper and two rollers. It is a minor invention, though very useful, and one by which, to use his own words, time, labour, and money are saved ; dispatch and accuracy are attained ; and secrecy is preserved. It has got into general use all over the world, and gives Mr. Watt another claim to the gratitude of mankind, if indeed the invention of the improved steam-engine could enlarge his claim.

Mr. Watt lost his first wife before he quitted Glasgow. She is said to have been an excellent woman, and by her he had a son and a daughter. The son has for a number of years carried on the steam-engine manufactory in partnership with Mr. Boulton's only son. They have only to tread in the steps of their fathers, to ensure them the esteem and good-will of mankind.

Soon after Mr. Watt settled at Birmingham he married Miss Macgregor of Glasgow, with whom he lived very happily. His domestic habits and those of that amiable woman perfectly agreed, and never was there more harmony in any family.

Mr. Watt was afflicted with a violent head-ache for many years ; but for a long period, we believe, he had been free from that complaint, and to the last he was a most cheerful and entertaining companion.

Mr. Watt used to relax from his study and intense thinking by reading novels ; but he did that, as he did every thing else, with measure and moderation.

The steam-engine is now employed more or less in every country in Europe, and lately three engines have been erected to drain some of the mines in South America.

The navigation of rivers has become much more expeditious by means of the application of the power of steam, and there are

hopes that it may in time be employed to advantage on the open ocean.

That the power of steam may be employed to the ploughing of land, is by no means improbable, and we believe there are at this time projects in embryo for that very purpose.

The number of horses kept for labour is already very considerably diminished; the prices of coal, iron, and the produce of mines of all sorts are also reduced, or prevented from augmenting, as they otherwise would have done; so that in every view of the matter the invention is highly beneficial to mankind.

The life of a sedentary or studious man produces few incidents; but in particular where, like Mr. Watt, he happens not to live amongst a society where he can associate much with others of a similar cast.

Birmingham, or the country round, afforded few men who were calculated to associate with Mr. Watt; accordingly he was almost constantly at home, and very seldom in company.

Mr. Watt had for a number of years quitted business, having acquired an independence, and having a son to continue the manufactory, as we have already said, with the son of Mr. Boulton.

Mr. Watt was a fellow of the Royal Society of London, and a member of the Institute of Paris; and there is not any body of learned or scientific men in Europe that would not have thought his being admitted into their number as an honour conferred upon them.

He died at the age of eighty-four, at his house at Heathfield, near Birmingham, having enjoyed his usual health and spirits almost to the last.

Those who knew him best, and were the most capable of appreciating his talents, esteemed him the most highly; and his was the rare felicity to have survived envy and closed his days in the possession and enjoyment of the esteem and admiration of his contemporaries. But the time is not yet come when full justice will be rendered to the wonderful effects his discoveries have produced in the world. The man, who by the strength of his genius under the greatest difficulties, brought to perfection a power that acquires for this country such incalculable benefits, and who, in remote antiquity, would have been deified amongst the inventors of the arts of life, has been suffered to depart this world without any notice by the rulers of his country: but it is to be hoped he has left friends who will do ample justice to his merit and his memory, which we have endeavoured to do as far as our means afforded and our space admitted.

LXXIII. *On the Effects of anointing the Stems and Branches of Fruit Trees with Oil, and on the means of destroying Insects.* By Sir G. S. MACKENZIE, Bart. *

THE trees in my garden having been much infested with insects in the year 1815, I became anxious to devise means to prevent their increase. I recollected to have seen an apple tree in the Duke of Buccleuch's garden at Dalkeith, which had been almost destroyed by what is called the *scaly insect* †; but which had recovered on the application of a mixture of oil, sulphur, and soot. It is well known, that oil is fatal to insects, and to this part of the mixture I attributed the recovery of the tree. I conceived that oil, applied to the stems and branches of trees, might act in two ways; it might destroy the eggs and pupæ of insects already deposited; and it might prevent the attack of insects in future. It occurred to me also, that oil, by softening hard and diseased portions of the bark, might be in these respects beneficial to the health and growth of the tree, and enable the vegetative power to throw off such portions by a natural process, which might be preferable to the more violent proceedings of scraping, while the bark is constricted.

With these views, I directed my gardener to anoint a considerable number of trees of different kinds. Not being aware of any injury which was likely to arise from allowing the oil to touch the buds, he zealously rubbed every recess into which it was possible for eggs to be deposited; and this has been the means of my discovering, more extensively than I should otherwise have done, the effects of oil, both in regard to benefit and injury; though those of the last kind have put me to some little inconvenience. I shall now detail these effects.

Apple-trees.—In every case where the buds were not touched, every beneficial effect has followed the application of oil to the stems and branches. Fruit-buds, if touched, are destroyed; and also, if far advanced, the leaf-buds. But new buds of both kinds are afterwards produced in great numbers; and I remarked on two young trees with long bare stems, that buds burst forth on the stems, where none had appeared before. This is easily accounted for. The sap not being able to find the usual outlet in the expansion of the buds formed the previous year, acted so as to produce new buds and branches, in the same

* From the Transactions of the Caledonian Horticultural Society.

† The *scale* has, I understand, been lately discovered to be the nidus, in which the eggs of some winged insects are deposited. Larvæ have been observed to issue from it, but they have not yet been found in the pupa state.

manner as it does when a tree is cut over. There are now numerous fruit-buds on trees which had but few, and those few were completely destroyed.

Pear-trees.—These, though the more advanced fruit-buds suffered, have been less injuriously affected than apple-trees. Their growth has been unusually vigorous; and great numbers of new buds have been formed, covering branches which before were naked. This I remarked particularly on a jargonelle.

Plum-trees did not appear to suffer in any respect, but shot out blossoms and wood vigorously.

Peach-trees.—One tree, an old one, appeared to have been totally destroyed. But on examining it carefully, I observed some buds which appeared alive. I cut all the branches down to these buds, which have produced astonishingly fine shoots. A young tree, which for a year or two had made so little progress, and appeared so unhealthy, that it had been condemned, has shot forth in a surprising manner, and has become a very handsome tree. I was not, at first, particular in my examination of this tree, as, from its former appearance, I was not anxious about it. It is probable, however, that the oil had not reached the best buds.

Apricot-trees were so much injured, that the shoots were feeble, and the trees ultimately perished. I do not yet know what the effect will be when the buds shall be carefully avoided when the oil is applied.

Cherry-trees were seriously injured. When I speak of injury, I mean, that in those cases where the buds were not spared, new ones did not push forth, and the general health and vigour of the tree seemed impaired.

Vines, treated in this manner, without sparing the buds, die down to the root, from which strong shoots are afterwards sent up. When the buds are not touched, they grow vigorously. But, on the whole, as the annual exfoliation of the bark admits of its being easily removed, I am not inclined to advise the application of oil to vines.

Gooseberry and *Currant-bushes* did not appear to derive any benefit, but seemed to me rather injured by the oil.

Those peach-trees which were not oiled, were as usual infested with aphides, while no insect of any kind was to be seen on those which had been oiled.

The apple aphid (*A. lanigera*) has been entirely extirpated from one garden, by means of oil applied to every part where it appeared; and I doubt not of its being soon destroyed in every district of the kingdom which it has reached, if the same means be used.

While the experiments leading to the results now detailed were

were in progress, I was informed, that a Lady, who is fond of horticulture, had cured several trees of canker, by first removing the diseased parts, and then covering the wound with a piece of rag spread with hog's lard. We may infer, that oil will have the same effect.

I noticed very early in the progress of my experiments, that on the stems and branches which had been oiled, parts diseased, and where branches had been removed, became of a different hue from the rest of the bark; an exfoliation appeared to commence; and in autumn, new bark was seen to have been produced, and to have displaced that which was dead and diseased, so that it could be easily removed.

From what has been related, I am satisfied, that in many cases, trees will derive much benefit from the application of oil, care being taken not to touch the buds, especially if they have begun to swell.

Besides the enemies to fruit, which lurk in the bark, we have yet to contend with those which deposit their eggs on the leaves and blossoms. From wall and espalier-trees, the larvæ may be removed with little trouble; but when an attempt is made to clear standards, the trouble and time required cannot be expected to be repaid. Gardeners are in the habit of suspending on wall-trees, bottles with a little sugar, or honey, and water, to destroy the insects which attack ripening fruit. If they will hang up the bottles early in spring, both on wall-trees and standards, and continue to use them during the whole season, they will be surprised at the destruction of insect enemies which will ensue. Thousands of insects, some in search of food, and most of them pregnant, and some moving about for the purpose of depositing their eggs, will be tempted to their destruction; and few, comparatively, will be left to do mischief in the autumn. The bottles I have used for this purpose, are narrow in the neck, and depressed a little under the shoulders.

There is an enemy, however, which does more mischief than perhaps all the others put together, that cannot be destroyed by these means. As this enemy commits its depredations only during the night, it is not probably generally known. Pear-trees and vines seem to be most liable to the attacks of this lurking enemy; but it has been detected on many other kinds of trees. The destruction so often observed on grafts, is the result of this creature being permitted to multiply. It is, I believe, the *Curculio vastator*, a weevil, which retires during the day amongst little clods of earth, from which, owing to its colour, it is scarcely distinguishable, as it never moves when touched. When mischief is observed to have been done, and the enemy is not perceived, by looking amongst the earth at the foot of the tree,

these weevils will be found. They can only be destroyed by searching for them, and killing each individual when discovered. The method which I have found the simplest and the most effectual, is to tread the earth round the stem after it is dark, and to place bits of slates, tiles, or small stones round it. In the morning, the weevils retreat under them, and may be picked up. Cracks in the bark, the separations of branches, holes in the wall, and every cranny into which it is possible for the weevils to enter, should be examined. Perhaps wrapping round the stem, a little tow soaked with some adhesive composition, such as the basilicon ointment of apothecaries, may have the effect of arresting these insects, when they attempt to ascend the stems of the trees.

The *Curculio abietis* was detected once on a vine. This is a considerably larger weevil than the *vastator*, and is one of those with a lengthened snout. If gardeners will take the trouble, occasionally, to look over their trees and bushes in the night-time, many nocturnal depredators, and their haunts, may be discovered, which are at present little suspected. It would be of importance, when any new enemy is discovered, that it should be sent to you, with an account of the manner in which it inflicts injury, of the particular parts of the plant which it attacks, and of such other circumstances as may be known concerning it.

A member of this Society, Mr. John Linning, has informed me, that he has found a little oil put on the stems of carnations, very effectual in protecting them from the attacks of earwigs.

As this defence is always at hand, and may be easily made use of, horticulturists and florists will, I hope, repeat and extend the experiments which have been made.

The oil which I used was common fish-oil. Whale-oil of the coarsest kind will answer equally well; and indeed any greasy substance. No more should be put on than will just produce a shining surface.

LXXIV. Notices respecting New Books.

Philosophical Transactions of the Royal Society of London, for 1819. Parts II. and III.

THE following are the titles of the papers contained in these additional Parts of the Transactions of the Royal Society for the present year:—On the Specific Gravity and Temperature of Sea Waters in different Parts of the Ocean and in particular Seas, with some Account of their Saline Contents. By Alex. Marcet, M.D.—An Account of the Fossil Skeleton of the *Proteo Saurus*.
By

By Sir Everard Home, Bart.—Reasons for giving the name of Proteo Saurus to the Fossil Skeleton which has been described. By the same.—Some Observations on the Peculiarity of the Tides between Fairleigh and the North Foreland; with an Explanation of the supposed Meeting of the Tides near Dungeness. By Captain James Anderson, R. N.—On the Ova of the different Tribes of Opossum and Ornithorhyncus. By Sir Everard Home, Bart.—The Results of Observations made at the Observatory of Trinity College, Dublin, for determining the Obliquity of the Ecliptic, and the Maximum of the Alteration of Light. By the Rev. J. Brinkley, D.D.—On some new Methods of investigating the Sums of several Classes of infinite Series. By Charles Babbage, Esq.—On the Optical and Physical Properties of Tabasheer. By Dr. Brewster.—An Account of a Membrane in the Eye now first described. By Arthur Jacob, M.D.—A new Method of solving Numerical Equations of all Orders by continuous Approximation. By W. G. Horner, Esq.—An Account of Experiments for determining the Variation in the Length of the Pendulum vibrating Seconds at the principal Stations of the Trigonometrical Survey of Great Britain. By Captain Kater.

Notice.—The 17th Number of Leybourn's Mathematical Repository, containing the Geometrical Solution of the Problem of inscribing a regular Polygon of seventeen Sides in a Circle.—This Number also contains several other curious mathematical Problems and their Solutions, as well as original Papers on Mathematical Subjects; together with the following Set of Questions to be answered in a future Number (XIX).

1. A and B travelled on the same road and at the same rate from H to L. At the 50th mile stone from L, A overtook a drove of geese, which were proceeding at the rate of three miles in two hours; and two hours afterwards met a stage waggon which was moving at the rate of nine miles in four hours. B overtook the same drove of geese at the 45th mile stone, and met the same stage waggon exactly forty minutes before he came to the 31st mile stone. Where was B when A reached L?

2. The curve AVR and semicircle APB have the same abscissa, the ordinate $MV = \tan \frac{1}{2} \text{ arc } AP$: prove that the area AMV = twice the segment AP.

3. A parabola revolves round its axis, which is vertical, in a given time, and the angular motion will just prevent a body at any point of the curve from descending: required the parameter of the parabola?

4. To divide a given arc (A), less than a quadrant, into two such parts (p) and (q) that $\tan^m p \times \tan^n q$ may = max.

5. A

5. A normal at any point of an equilateral hyperbola is equal to the distance of that point from the centre. Required the demonstration.

6. C is the centre of an ellipse and F either focus, PH a tangent at P: draw the diameter PCp and pF meeting PH in H. Prove that pH = the transverse diameter.

7. P is any point in the diameter of a circle and PB a perpendicular to the diameter: draw any chord PA, and a tangent AB meeting PB in B, and draw BD and CE (C the centre) perpendicular to PA; then $PE = DA$. Required a demonstration.

8. Within a given triangle suppose another triangle to be inscribed, by joining the middle points of its sides; and again, within this triangle suppose another triangle to be inscribed by joining the middle points of its sides, and so on *ad infinitum*: What will be the limit of the aggregate of the sum of the squares of all the sides of all the triangles so formed?

9. Let any right line be drawn through the focus of a given conic section, terminating in the curve; then a fourth proportional to the whole line and the two segments thereof, made by the focus, will always be of the same constant length. Required a demonstration.

10. A triangle being given, it is required to describe three circles, so that each circle shall touch the other two and a side of the triangle at the point of bisection.

11. Required the curve that has at each point the radius of curvature a fourth proportional to the abscissa, the ordinate, and a given straight line.

12. To determine the nature of the curve such that the perpendicular from a given point upon the tangent shall be a mean proportional between a given line and the segment of the axis intercepted between the tangent and this same given point.

13. To determine the equation to the curve whose tangent is a mean proportional between the segment of the axis intercepted between it and a given point, and that same segment augmented by a given line.

14. A body, urged by a force perpendicular to the horizon, describes the quadrant of a circle. Required the law of force which will make it recede uniformly from the horizontal radius, and the time elapsed and the velocity acquired at any point of the descent.

15. The characteristic property of the circle is, that all the chords which pass through a certain determinate point in its plane are equal: but there exists an indefinite number of curves which possess this property. It is required to find the most general equation to curves of this nature.

16. A given rod or beam has one end suspended by a cord of a given

a given length, fixed at a given point above an inclined plane of a given inclination; it is required to determine the position of the beam, weight sustained by the cord, and pressure against the inclined plane, when the beam is in equilibrio.

17. Find the rectification and quadrature of the magnetic curve, which is such, that if lines be drawn from the poles to any point in the curve, and ϕ and ψ be the angles they make with the line joining them, $\cos \phi + \cos \psi = c$, a constant quantity.

18. Required a number consisting of six digits $abcdef$ such that, its multiples by 1, 2, 3, 4, 5, 6 may contain amongst them the following arrangements:

a	b	c	d	e	f
b	c	d	e	f	a
c	d	e	f	a	b
d	e	f	a	b	c
e	f	a	b	c	d
f	a	b	c	d	e

19. It is well known that the formula $x^2 + x + 41$ contains a great number of primes: prove that when x is of either of the two following forms it cannot be a prime, $x = 53a + 3$ and $x = 97a - 8$.

20. If the beam in question 401 instead of resting upon the curve, be at liberty to slide freely over the prop and descend by the force of gravity, it is required to determine the motion of its centre of gravity.

Preparing for Publication.

Mr. Accum has in the press, A Treatise on Adulterations of Food, and on Culinary Poisons; exhibiting the fraudulent sophistications of Bread, Wine, Water, Tea, Coffee, Cream, Spirituous Liquors, Cheese, Mustard, Vinegar, Olive Oil, Pepper, Confectionary, and other articles employed in domestic Economy, and Methods of detecting them. The work will form one volume twelves, containing upwards of 300 pages, and will be published in the course of next month.

A Translation of M. Lennec's excellent work on the Diseases of the Thoracic Viscera and on Auscultation. By Charles Thomas Haden, Surgeon to the Chelsea and Brompton Dispensary, &c.

Lectures on General and Medical Botany. By Anthony Todd Thomson, F.L.S. Member of the Royal College of Surgeons in London, &c. &c.

Mr. Cross will shortly publish a concise History of the Variolous Epidemic which occurred in Norwich in the year 1819, with an Estimate of the Protection afforded by Vaccination, &c.

An

An Elementary Treatise on Mechanics, by W. Whewell, M.A. Fellow of Trinity College, Cambridge, vol. 1. containing Statics and Part of Dynamics.

M. Charles Dupin is preparing for publication An Account of his Travels in Great Britain during the years 1816, 1817, 1818, and 1819, undertaken with a view to information on the subject of our Military and Marine Establishments, Bridges and High Ways. It will form six volumes in quarto, and be accompanied with an Atlas of Plates.

LXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THIS Society resumed its sittings on the 4th of November, by commencing the reading of the Croonian Lecture, by Sir E. Home, entitled "A Further Investigation of the component Parts of the Blood."

Nov. 11. The reading of Sir Everard's Lecture was concluded, in which he attempts to show that smaller globules and of a different nature from those commonly supposed to exist in blood, are found in it. They were first observed by Mr. Bauer while examining the layers composing an aneurismal tumour. They were seen in the coat in contact with the circulating blood, in the proportion of 1 to 4, compared with the larger globules; but in the other layers they were more numerous, and in that which had been first formed they existed in the proportion of 4 to 1. Their estimated size was $\frac{1}{2800}$ of an inch. Crystals of muriate and phosphate of soda, and sulphate of lime were found in making a section of another aneurismal tumour: these and the globules before mentioned Sir E. supposes to have originally existed in solution in the serum; the globules being to be seen only after the blood has coagulated. In coagulated lymph, found during high inflammation, these globules were observed mixed with a few colourless blood globules. They were also found in great numbers in the upper firm coat of the buff of blood, while the lower and softer parts consisted chiefly of blood globules. The author proposes to call the new globules by the name of globules of *lymph*, to distinguish them from blood globules. The author found the quantity of carbonic acid gas evolved from buffy blood under an exhausted receiver to be much less than that from healthy blood; and that by far the greatest quantity of this gas was yielded by blood drawn from a healthy person an hour after a full meal.—Both lymph globules and blood globules were found in the mucus of the pylorus and duodenum. In chyle the size of the globules is various. Mr. Bauer supposes that the blood,

globules

globules are formed in the mesenteric glands, with the exception of the colouring matter, which they obtain on exposure to air in passing through the lungs.

The Bakerian Lecture by Mr. Brande was next commenced reading—"On the composition and analysis of the Inflammable Gaseous compounds resulting from the destructive distillation of coal and oil, with some remarks on their relative heating and illuminating powers."

Nov. 18.—The Bakerian Lecture was concluded. In the first part of this lecture Mr. Brande attempts to show that no other compound of carbon and hydrogen can be demonstrated to exist than that usually called olefiant gas, consisting of one part of carbon and one of hydrogen; and that what is usually called carburetted hydrogen is in fact a mixture of hydrogen and olefiant gases. The truth of his opinion is supported by the author by various experiments on gases obtained from coal, oil, and other substances. He supposes that many of the products obtained by destructive distillation of coals, &c. are of secondary formation, and result from the mutual action of the gases first formed. A peculiar compound of hydrogen and carbon is thus formed, by passing pure olefiant gas through a tube containing red-hot charcoal, similar in appearance to tar, but possessing the properties of a resin. Sulphuret of carbon was formed by the mutual action of sulphuretted and carburetted hydrogen. Some new modes of analysing gaseous mixtures were also pointed out in the first part.

The second part related to the illuminating and heating powers of coal gas, and of oil gas. The illuminating powers of olefiant, oil, and coal gases are stated to be to one another nearly as 3, 2, and 1; and the ratio of their heating powers to be nearly similar, that from coal being greatest, and that from olefiant gas the least. In this part the advantage in point of illumination by employing many jets in the burners (instead of one) placed so near each other that the different flames can unite, was illustrated by experiments.

The lecture concluded with comparative experiments on terrestrial and solar light. Gas lights, even when concentrated so as to produce a sensible degree of heat, do not affect the colour of muriate of silver, nor a mixture of chlorine and hydrogen gases; while the concentrated brilliant light of charcoal submitted to galvanic action not only affects the muriate but causes these gases to unite—sometimes with explosion. Concentrated moonlight did not affect either of these tests.

A paper "On the Elasticity of the Lungs," by Dr. Carson, was commenced reading.

Nov. 25.—Finished the reading of Dr. Carson's paper.

ROYAL ACADEMY OF SCIENCES AND BELLES LETTRES OF
BRUSSELS.

In the Phil. Mag. for August 1818, mention was made of three questions proposed by this Academy for competition, in the Class of Sciences during the year 1819.

On the first question only one Memoir has been presented, written in Dutch. The Academy after a rigorous examination of it are of opinion, that though it bears marks of being written by a very learned geometrician, it has not furnished a satisfactory solution of the question; that is to say, that the author has not succeeded, 1st, in proving that the hypothesis adopted by Euler is really a physical hypothesis. 2d, in presenting under what can be considered as a new form the beautiful theory resulting from it—nor 3d, in assigning the reason truly physical, Why a square plane with the weight P. attached to its centre exerts an equal pressure on the four supports placed at its four angles. Considering, however, that the Memoir has cost a great deal of labour, though unfortunately too foreign in general to the question proposed, and that it is partly a commentary on the beautiful Memoir of Euler, and partly a mathematical factum, in which are collected almost all the arguments which can be advanced in its favour,—the Academy have voted to the author a silver medal of encouragement.

On opening the billet which accompanied this Memoir it was found to be the production of Col. Huguenin, Director of the Cannon Foundry at Liege.

On the two other questions proposed by the Society at the same time, no papers have been offered, and the Society have resolved to abandon them.

LXXVI. *Intelligence and Miscellaneous Articles.*

BORACIC ACID.

It has been known for some time that Boracic Acid is found in solution in certain lakes in Tuscany. M. Robiquet (in the *Journal de Pharmacie*, 1819) states that M. Dubrouzet, one of the proprietors of the lakes in Cherchaio, informed him that he had obtained about two per cent. of this acid by evaporating the waters, and offered him any quantity of the acid, delivered in Paris at 3 francs the kilogram. Indeed considerable quantities have been sent to Paris, but hitherto without finding purchasers. The acid is in small scales of a greyish colour: its taste slightly bitter; its aqueous solution reddens litmus. It is not precipitated by either nitrate of silver or oxalate of ammonia; but strongly by muriate of

of barytes : it therefore contains an alkaline sulphate dissolved in water, it leaves an insoluble residue not acted upon by the mineral acids : this residue consists of different earthy bodies, mixed with a little oxide of copper. M. Robiquet suggests the adding soda, in due proportion, and converting this acid into borax, as a speculation likely to prove profitable.

GEHLENITE, NEEDLE-STONE, AND DATOLITE.

Dr. E. D. Clarke has lately detected potass in this stone. The property of forming a jelly in acids belongs to but few minerals, and the Doctor had long suspected that it was owing to the presence either of an *alkali* or an *alkaline* earth in stones containing silica. There seems to be no exception but where *zinc* or *lime* is present with the silica. In the instances of *Needle-stone* and *Datolite*, which both yield a transparent jelly when acted on by acids, and both contain lime, he has also detected *Soda*.

TITANIUM FOUND IN OXIDULATED IRON ORE.

M. Robiquet has lately detected titanium in the oxidulated octoedral iron from the steatite of Corsica. This ore, dissolved completely in muriatic acid, then evaporated to dryness in a moderate heat, and re-dissolved in water, leaves a white pulverulent substance, which when fused with potash, and afterwards dissolved in muriatic acid, gives all the characters of a solution of titanium. In this manner, six parts have been separated from 100 of the mineral; and M. Robiquet is inclined to believe that titanium generally accompanies the oxidulated iron in nature, and that this compound is not, as has been thought, peculiar to volcanic countries.—M. Berzelius found titanium in Elba iron ore.

POISONS.

A correspondent suggests that a complete work on poisons, especially those frequently met with, with their proper specifics when such are known, is a desideratum in the healing art to which medical authors should turn their attention. The experiments of Dr. Orphila in Paris promise much valuable information. It is now ascertained that sugar taken in lumps is a certain antidote for verdigris; that vinegar counteracts the dangerous effects of alkaline substances; and that raw albumen (white of eggs) if administered in time is a remedy for mercury sublimate.

EGYPT.

[Continued from p. 396.]

“Cairo, March 4, 1819.

“In our return to Thebes, we took in Ombos, Hagar, Tileit,
and

and Hemsrisha; Trenchis and Edfir we had seen coming up. Ombos is very late, and very bizarre in its construction; five pillars in front, and two entrances into two separate and parallel ranges of apartments. Hematis is a small and curiously enriched temple, originally unfurnished. Euneh, celebrated for its zodiac, and unquestionably the finest portico in Egypt, is nothing but a portico; the body of the temple is buried, and the town built on its roof. Edfir, in the same manner incumbered, is the most entire in its accessories, at least of any other Egyptian temples; it has all the accompaniments of propyla, area, inclosure, &c. quite perfect: but all sunk in Thebes. I am literally afraid of saying any thing of its gigantic size; a size not only of extent, but mass. The disjecta membra occupy many miles, and the largest temple, that of Kamek, is 1200 feet in length alone, without comprehending its dependent sacella temples, to the number of fifteen or twenty, with which it is surrounded. I do not remember a more positively sublime effect produced by any architecture I have ever seen, than the vista from the obelisk down the great portico.—I cannot say how much it struck and astonished me, when I suddenly turned round, without any preparation from former travellers, and unexpectedly came upon the whole forest of its pillars, enormous fragments of moss lintels, &c. I shall well and long remember it: if I had seen nothing else in Egypt, this would have repaid me for the worst part of the Egyptian tour, the seventeen days voyage to Alexandria.—We occupied so much time, as you may easily imagine, in taking the measurements and plans of this city of temples, that we had less time, perhaps, than was necessary, for the Tombs of the Kings; but I contrived, notwithstanding our hurry, to spend two entire days there—a place of wonders, half seen, half lost; and perhaps irrecoverably so, in the darkness of their strange emblems and language; but it is sufficiently impressive to confound and humble modern conceptions. Most of these tombs are a series to long strait excavations in the limestone rock, some of the length of 100 feet in galleries, opening into rooms, and terminating in the large arched sepulchral chamber, where the body of the monarch was deposited in a granite or alabaster sarcophagus. The Sarcophagus is already on its way to England. The sight of the stuccoed painting which I have got, will give you a better idea of its unaccountable preservation, than any vague terms could possibly do. It is not only fresh, but fresher than any wall painting I have ever seen at home after the first week. It is probable that its date is very ancient, and may exceed, if Herodotus's Persian Chronology may be relied on, 1000 years before Christ. All the injury it has received, has been subsequent to its late opening; the excessive rain for a day and a night at Thebes, almost proved

a miracle (Herodotus justly regards the same phænomenon in his time) has damaged the painting and sculpture at the entrance. We left Thebes late, and ran as fast as we could for Cairo; reports of the plague had reached us, and apprehensive, in case they proved true, of being detained in the country till the heat of summer, before which it could not possibly cease, we decided on sacrificing any ulterior or former projects, such as a journey to the Great Cairo, to the Faccem or Lake Moris, to these salutary and essential precautions. On our arrival here, we found that some accidents had occurred, of course in Alexandria, where its return is now periodic, but had not extended further, so that we were only allowed the time for the Holy Easter of Jerusalem and Mount Sinai; for my own part I have seen so many of these displays, and am now so little affected by any thing which is mere display, that were my companions originally willing, I should have preferred seeing objects which can communicate information; I should prefer a prosecution of our journey a little further into Arabia, to the sight of all the pilgrims from Godfrey de Bouillon down to Chateaubriand. On these occasions, however, I remember the advice of Terence, and am satisfied with a circuitous route. We remain a few days at Cairo, and have engaged camels for next winter; we of course travel in June. You may guess what our appearance may be, when some of my companions have expended nearly two hundred pounds on their costume. I am happy to say I have not been one of the fortunate. We travel twelve or thirteen in a party, including servants, interpreters, &c. Easter occurs the 16th, I think, of April: to-day is the 4th of March. We expect to have sufficient time to see the city during the week, and shall set off immediately after for Damascus and Palmyra, or Teduon in the Desert. I doubt the practicability of a tour in Asia Minor after this, as the heats are dangerous on the whole of the coast at a very early season, and I am not so Quixotic an errant as to risk my health so obviously, as I should do by travelling there after July. You may therefore reckon as a certain thing my return, somehow or other, to Smyrna before the first of the month. I may go once more to Attica, where my baggage has been sent from Constantinople, should I find a ship of war, and thence to Malta for quarantine. We have already had many kind offers from the officers on this station, to take us thither. I propose being in Sicily in October, and making the tour of the island before the end of the month. I shall take Calabria, and the Volscian and Samnite country in my way back to Rome."

NEW ISLAND IN THE BAY OF BENGAL.

A new island has been lately formed in the upper part of the Bay of Bengal, by a rapid accretion of the alluvium or soil, made

along the shores of the large rivers of the Indian continent. The island is nothing at present but a sand-bank ; but it is continually receiving such additions as will gradually render it a spacious tract. It was not visible four or five years ago, and it was only discovered, together with the canal, by vessels trading to Saugur, about the latter end of 1816. The situation is 21 deg. 35 min. of latitude, and 83 deg. 20 min. of longitude, east of Greenwich ; this position is precisely that which had been indicated in the maps as the Bank of Saugur, at the eastern extremity of the upper part of the island of that name. Its formation between the mouths of the Houghly and the canal off the bay, may well enough account for its origin. There being two considerable mouths of rivers, with rapid currents running into the sea, both east and west ; these must have long been a submarine agglomeration, which has now risen above the surface of the ocean, and must increase under the protection of the continental lands that lie between those two vast arms of the Ganges.

It may be about two miles in length from east to west, and half a mile wide from north to south.—At the western extremity are little elevations that command a view of the sea. The centre of the island rises high enough to afford shelter, except during the violence of a tempest. The south shore consists of a fine solid sand, with a gentle declivity ; one of its bays lies very convenient for such as had a wish for sea-bathing.

In some parts the island is covered with the dung of birds, which becomes a kind of manure for the soil. Myriads of small crabs cover the northern coast, and their visits are productive of some utility. The central part of the island looks at a distance like a green lawn, dazzling to the view : herbage has taken root here, and there are a number of tufts of long *cass* (*saccharum spontaneum*) that thrive very well.

In short, the soil has every appearance of becoming well adapted for all the purposes of vegetation ; and there can be little doubt that what is now the sandy base of the isle, will hereafter contain produce like the neighbouring islands and continent ; and that this spot, where man now roves unrestrained, will, at no very distant period, conceal the haunts of even the savage tyrant of the neighbouring forest.

At present the island is only visited by wood-cutters and fishermen, who have raised two huts on it in honour of Siva, an Indian divinity. There is no vestige of any other habitation. The canal that separates the island from Saugur is well stocked with fish of different descriptions ; and the southern shore is frequented by tortoises.

THE ADRIATIC.

A very extensive survey of the shores of the Adriatic has been completed by Captain Smith of the *Aid* frigate. Several Austrian officers were employed at the same time, who have proceeded to Vienna with the result of their labours.

NORTHERN EXPEDITION.

The *Hecla* and *Griper*, by the last accounts received, had reached 86°. [Is there no mistake in this?] In Baffin's Bay they had fallen in with an immense mass of ice, which appeared to be formed upon a solid rock. The sea to the north of this huge mass presented the appearance of a lake perfectly free from ice. None of the inhabitants that they met with seemed to have seen or heard of the former expedition under Captain Ross. Report says that it seems to be the opinion of these voyagers that there is no northern outlet from Baffin's Bay.

The bottle, No. 2, thrown overboard by Captain Ross, of His Majesty's ship *Isabella*, on the 2d of June, 1818, lat. 65. 40. N. long. 54. 10. W. of Greenwich, to ascertain the direction of the current in Davis's Straits, was found by one of the servants of A. Macdonald, Esq. at Balranald, North Uist, on the 17th of July last, and the paper inclosed in it quite dry; so that it was 13 months and 14 days on its passage; the latitude of Balranald is about 57. 20. N. The paper has been transmitted to the Admiralty, agreeably to their request.

EARTHQUAKES.

Corfu, 11th Sept.

On the 4th of this month, at 9 o'clock in the evening, we had here such a violent shock of an earthquake that in an instant the bells of all the churches began to ring. As this happened in forty churches at once, it may be supposed what horror was excited; the inhabitants rushed out of their houses, and several buildings were damaged; the air was quite serene, and the moon shone bright. We expect now that we shall hear of an eruption of Vesuvius or Etna, as earthquakes in this country are usually ascribed to such eruptions.

Comrie, Perthshire, Nov. 29.

Yesterday morning, about half-past one, this place was visited by one of the most alarming shocks of an earthquake felt here for ten years past. It not only awakened the people, but its violence made some instinctively leap from their beds and run to the door before they were aware of the cause of their panic. The convulsion, accompanied with the usual hollow grumbling noise, resembling the sound of distant thunder, continued for about ten seconds, occasioning, while passing immediately under

us, the crashing of the timber in the houses, moving of the chairs, jingling of the fire-irons, glasses, &c. &c. It was felt for several miles round the village, and seemed to commence in the north-west, passing the village and its vicinity in a south-easterly direction, where it subsided.

ON THE NAUTICAL ALMANAC.

To Mr. Tilloch.

Dec. 9, 1819.

SIR,—A writer in the last Number of your valuable Miscellany has made a serious and a very proper charge against the publishers of the *Nautical Almanac*, for not having a sufficient number of copies printed for the use of the public. In addition to the inconveniences arising from that circumstance, I would draw the attention of the public to those which arise from another and a similar source. It has been publicly stated that there is a printed list of the errors in the *Nautical Almanac* for the ensuing year (1820), which extends to four pages!!! On this information I immediately sent to the publisher for that list; but was informed "that there were not any of them left." Now, sir, whether the publisher intends to favour the world with a reprint of that list, I do not know: but, I hope, after this hint, that the conductors of that national work will have ready in his shop, for the use of the public, not only a sufficient number of copies of the *Nautical Almanac* itself, but likewise the several lists of *errata* which, to the discredit of that work, it is now found so necessary to provide. The Board of Longitude contains some of the most respectable names in the country; men of honour and of science; and who cannot be supposed indifferent to any negligence on this subject. I am, sir, your obedient servant,

PHILASTER.

WHITE'S EPHEMERIS.

In this Almanac for 1820 a typographical error occurs which may for a moment perplex those who use it, if not attended to. Throughout the year the symbol for the Sun is placed at the head of the Heliocentric longitude of the Earth instead of the mark for the Earth. It now stands, Heliocentric Long. of the *Sun*!! The mark for the Earth is omitted in the "*Astronomical Characters explained.*"

NEW PLANET.

The numerous recent observations of new comets have been matter of surprise to astronomers. Their number however will be now lessened. An astronomer of some eminence having lately been occupied in arranging and reducing their orbits into order,
has

has unexpectedly found several of them to correspond with one and the same planetary orbit. Thus have we lost some of our comets, but found a new star. The particulars shall be laid before our readers in a future Number.

NEW COMET.

A new comet was discovered on the 28th Nov. by M. Blanpain, at Marseilles, in the south wing of the constellation *Virgo*. Its angular diameter appeared to be about six or seven minutes—the germ of a very small and confused nucleus has been observed, but no tail whatever. The following observations have been made: On the 29th of November, at 10 minutes past six in the morning, true time, 138 deg. 7 min. right ascension, 3 deg. North declination.—On the 30th, 45 minutes past five in the morning, right ascension, 184 deg. 1 min. North declination 1 deg.—On the 2d December, at six minutes past five in the morning, right ascension 185 deg. 1 min. North declination 2 deg. 3 min.

EARLY SEVERITY OF THE WINTER.

A letter from *Keswick* in Cumberland, dated on the 28th ult. says, “The weather for twelve years past has never been so severe in November, as of late. On the morning of Tuesday the 23d, the thermometer here was down to 25° F., and continued at 27° through most of the day; whereas, last winter, the same thermometer was never noted below 31°. Snow first fell on the tops of the hills visible from hence, as early as the 4th of October!: on the 21st a more extensive fall of snow took place, and continued at intervals until noon of the next day, accompanied by a smart frost! A thaw followed, but on the 25th snow again fell, and harder frost succeeded, which on the night of the 28th was so severe as to *frecze the milk* in several farmers’ houses, in the higher valleys of this district; on the afternoon of the 29th, the frost and snow left the valleys, but the latter continued to clothe the hill tops until the 4th ult.; warmer and rainy weather succeeded until the 16th, when the frost had again returned and lasted till the 19th, with snow on the hill tops, which has lain to the present time, and seems now likely to continue through the winter: some rain, in the valleys, and a high wind succeeded on the 19th and 20th; on the 21st the frost commenced, which still continues, and on the 26th the snow fell, deep, in the valleys as well as on the hills, which still continues.”

ACCIDENTS AT COAL PITS.

On the 9th of December as four sinkers were opening out a staple in the Old Engine-pit, to the Hutton-seam, at East Rain-
G g 3 ton,

ton, belonging to Lord Stewart, the gas ascended the staple and suffocated William Osley, one of them. Another buckled himself to the clasp, and was drawn to bank. A corf was let down, and the other two fortunately got into it, and were brought up almost dead. The gas ascended the shaft of the pit, was ignited at the engine, which also took fire, and continued burning till between ten and eleven o'clock at night, when the gas exploded in the pit. The shock was felt three or four miles; the wood-work of the engine was all burnt.

NEW METHOD OF GRAFTING TREES.

A common method of grafting, is by making a transverse section in the bark of the stock and a perpendicular slit below it: the bud is then pushed down to give it the position which it is to have. This method is not always successful; it is better to reverse it, by making the vertical slit above the transverse section, and pushing the bud upwards into its position—a method which rarely fails of success; because as the sap descends by the bark, as has been ascertained, and does not ascend, the bud thus placed above the transverse section, receives abundance, but when placed below, the sap cannot reach it. *Annales de Chimie*, xi.

TO PROTECT TURNIPS FROM THE FLY.

Experiments made by Lord Thanet and Mr. Grey have convinced them that lime sown by hand, or distributed by a machine, is an infallible protection to turnips against the ravages of the fly. It should be applied as soon as the turnips come up, and in the same daily rotation in which they were sown. The lime should be slacked immediately before it is used, if the air be not sufficiently moist to render that operation unnecessary. These gentlemen have communicated the foregoing fact to the Board of Agriculture.

LIST OF PATENTS FOR NEW INVENTIONS.

A Grant unto Henry Tritton, Esq. of Battersea, Surrey, for his new method of producing rotatory motion.—4th Dec. 1819.

To James Dickson, lapidary, of Gilmore-place in the county of Edinburgh, for improvements in communicating power to machinery by water, spirits of wine, quicksilver, oil, or fluids; which improvements are applicable to other useful purposes.—4th Dec.

To Samuel Lambert, of Prince's-street, Leicester-square, Middlesex, laceman, who, in consequence of a communication made to him by Charles Augustin Busby, now residing at New York, in the United States of America, is in possession of an invention for an improved water-wheel, applicable to mills and navigable

navigable bodies, and for other improvements also applicable to mills and navigable bodies.—4th Dec.

To Henry Constantine Jennings, of Carburton-street, Saint Marylebone, Middlesex, Gent. for his substitute for pitch.—4th Dec.

To William Feuillade, of Mortimer-street, Cavendish-square, Saint Marylebone, Middlesex, Gentleman, for his improved mechanical apparatus, instrument, or machine, (intended by him to be called An Aid-form) for the prevention and remedy of deformity and ill shape in the trunk or body parts of human beings.—4th Dec.

To Sir William Congreve, of Cecil-street, Strand, Middlesex, Bart. for certain improvements on the manufacture of bank note paper for the prevention of forgery.—4th Dec.

To William Rodger, of Suffolk-street, Charing Cross, Middlesex, lieutenant in the Royal Navy, for a substitute for anchors, which he intends to denominate A Block Anchor.—4th Dec.

To William Carter, of Grove-place, Paddington, Middlesex, for certain improvements in the manufacture of measures of capacity.—4th Dec.

To James Lee, of Merton, Surrey, for certain machinery and a process for breaking, cleaning, and preparing flax and hemp for use, and which is also applicable to other vegetable fibrous substances.—13th Dec.

To James Wood, of New Compton-street, Saint Giles's, Middlesex, for an improvement in the formation and position of the long keys B natural and C sharp used upon the musical instrument commonly called the clarionet, for the more easily fingering of the same.—18th Dec.

To Apslèy Pellat, Jun. of Saint Paul's Church-yard, London, for encrusting into glass vessels and utensils, white or other coloured, printed or otherwise ornamented figures, arms, crests, cyphers, and any other ornaments made of composition metal, or other suitable material.—18th Dec.

To Thomas Dehany Hall, of Park-place, Regent's Park, Middlesex, for an improved method of dyeing cloths and other substances, and of preparing dyes for that purpose.—18th Dec.

To James Henry Lewis, of High Holborn, Middlesex, for his improvement or substitute for or addition to pens as usually employed in the art of writing, which are denominated Caligraphic Fountain Pens.—20th Dec.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1819.	Age of the Moon.	Thermo- meter.	Baro- meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
Nov. 15	27	46°	29.50	Cloudy
16	28	42.5	29.43	Ditto
17	new	45°	29.70	Ditto
18	1	44°	30.05	Fine
19	2	40°	29.88	Ditto
20	3	36°	29.40	Cloudy—rain at night
21	4	41°	29.04	Ditto
22	5	36°	29.40	Fine
23	6	35°	29.72	Ditto
24	7	41°	29.80	Ditto
25	8	39°	29.85	Ditto
26	9	34°	29.63	Cloudy
27	10	36°	29.80	Fine
28	11	33°	29.77	Foggy—rain at night
29	12	48°	29.40	Rain
30	13	52°	29.40	Cloudy
Dec. 1	full	47°	29.70	Ditto
2	15	47°	29.72	Rain
3	16	39°	30°	Fine
4	17	47°	29.40	Cloudy—heavy rain P.M.
5	18	41°	29.94	Ditto
6	19	38°	30°	Ditto
7	20	36°	30°	Ditto
8	21	29°	30.14	Stormy
9	22	27°	30.03	Fine
10	23	29.5	29.90	Ditto
11	24	32°	29.90	Ditto
12	25	34°	29.84	Ditto
13	26	34°	29.84	Rain and snow
14	27	30.5	29.55	Fine

N.B. The weather has been more severe the last week than it was at any time last winter.

METEOROLOGICAL TABLE,
By MR. CARY, OF THE STRAND,
For December 1819.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock Morning.	Noon.	11 o'Clock Night.			
Nov. 25	32	39	35	30.04	16	Fair
26	37	41	35	29.77	11	Cloudy
27	30	36	32	.92	7	Cloudy
28	31	33	45	.87	7	Cloudy
29	47	52	50	.67	0	Rain
30	50	52	50	.65	10	Cloudy
Dec. 1	48	49	40	30.02	14	Fair
2	46	49	40	30.00	0	Rain
3	35	41	38	.23	15	Fair
4	46	49	42	29.62	0	Rain
5	42	42	36	.97	10	Cloudy
6	37	37	36	30.10	10	Cloudy
7	36	36	36	.04	0	Cloudy
8	32	28	25	.10	12	Fair [the evening
9	26	27	28	.07	0	Cloudy—Snow in
10	27	33	23	29.97	0	Cloudy
11	16	27	24	30.05	10	Fair
12	30	37	31	29.95	6	Fair
13	26	35	26	.80	0	Cloudy
14	24	34	30	.67	8	Fair
15	30	37	33	.50	6	Cloudy
16	30	37	30	.95	9	Fair
17	40	44	49	.52	0	Rain
18	51	54	50	.50	6	Fair
19	49	52	52	.85	0	Cloudy
20	52	54	51	.72	0	Small Rain
21	47	47	51	.77	0	Rain
22	52	52	50	.75	8	Fair
23	50	41	37	.46	10	Fair
24	32	37	32	.35	10	Fair
25	31	33	30	.43	16	Fair
6	25	32	30	.55	12	Fair

N.B. The Barometer's height is taken at one o'clock.

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The Paper of a Naval Surgeon on Damp Decks, &c. came too late for Insertion this Month.

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